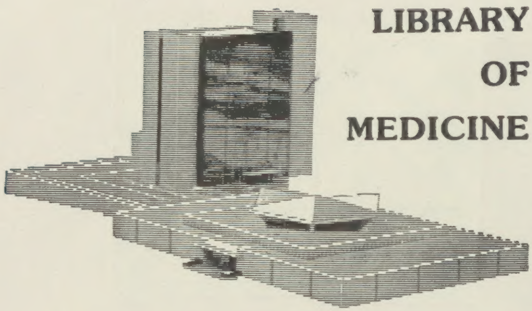


MARY PUTNAM JACOBI, M.D.

A PATHFINDER IN MEDICINE

WITH SELECTIONS FROM HER WRITINGS
AND
A COMPLETE BIBLIOGRAPHY

EDITED BY
THE WOMEN'S MEDICAL ASSOCIATION
OF
NEW YORK CITY

G. P. Putnam's Sons
New York & London
The Knickerbocker Press
1925

The Knickerbocker Press
G. P. Putnam's Sons
New York

Made in the United States of America

To

ELIZABETH BLACKWELL, M.D.

"Among all the pioneer group of women physicians, hers chiefly deserves to be called the Record of an Heroic Life."

Women in Medicine—Mary Putnam Jacobi, M.D.

FOREWORD

THE Women's Medical Association of New York City, desires to perpetuate the memory of the work done by one of its founders, one of the great pioneer women in medicine. She opened the doors of a great university that women might equally with men obtain a scientific medical education. All her life she was a zealous worker for this advancement of the medical education of women. To continue this, her work, the Association has founded the Mary Putnam Jacobi Memorial Fellowship, thus far awarded four times, to increase the medical knowledge of the recipients. The Association in this volume has collected some of her medical writings, illustrating her studies on the medical problems of her day. With her writings as with her other medical work, "she was never satisfied. There was always a better than her best, a higher than her highest to be striven for; and in this striving she was not influenced by personal ambition, but by the higher object—the truth to be attained."

CONTENTS

Contents

MARY PUTNAM JACOBI
A Pathfinder in Medicine

MARY PUTNAM JACOBI

PHYSICIAN, TEACHER, AUTHOR

A diploma from the Female Medical College of Pennsylvania, in 1864, marked the entrance into the medical profession of Mary C. Putnam.[1] A year as interne in the New England Hospital followed.[2] The desire for a medical education absolutely unattainable in the United States, led her, as it has always led pioneers, "through tangled underwood of old traditions, out to broader ways," even to the doors of the Paris l'École de Médecine, to which women had not been admitted. The romantic story of this adventure, beginning in 1866, she relates in her letters from Paris to her beloved mother.[3] She was the first woman to be admitted to the French school. Her thesis, written during the Siege of Paris in the Franco-Prussian War, received the bronze medal.[4]

[1] The Female Medical College of Pennsylvania was founded in 1850, "and after a long and precarious period of struggle, finally touched upon a solid basis of medical realities and thence began its prosperous modern career."

The title of the college was, in 1867, changed by decree of court to that of the Woman's Medical College of Pennsylvania. It is now (1924) the only medical college in the United States devoted exclusively to the education of women in medicine.

[2] The New England Hospital, founded in 1862, the second hospital in the United States conducted by women physicians.

[3] Volume I, *Letters*.

[4] The copy of the thesis in the New York Academy of Medicine, was presented to the New York Hospital Library, April 23, 1892, by Francis D. Buck, M.D., and passed to the Academy of Medicine with the library of the Society of the New York Hospital, March, 1898.

Returning to New York in September, 1871, with what Sir William Osler described as "a Paris medical degree and a training in scientific medicine unusual at that date even among men," she immediately opened an office in her father's house, 328 East 15th Street, and began private practice. She entered at once into the professional life of the city, and joined in the discussions of those medical societies to which women were admitted.[1] The first such discussion seems to have been at a meeting of the Medical Library and Journal Association.[2] It was before this

---

News items, *Med. Record*, 1871 (September 15), page 335:

Mary Putnam

"Miss Putnam," says a Paris paper, "the young American who has for some years been following the course in l'École de Médecine, submitted her graduating thesis to the Faculty. It was read in the large lecture room of the College before a numerous audience, and was received with warm commendation. The President of the Board of Examiners found it deserving of the highest note—'extrémement satisfait.' This mark is rarely given for a thesis. Miss Putnam has also received the highest mark at each of her five examinations. She was ready for graduation a year ago, but the war broke up the schools, and she has devoted the year to work in the hospitals. She is the first woman who obtained admission to l'École de Médecine, but not the first who graduated, as Miss Garret took a year's course and received her degree a year or more ago. She writes that one of the dedications of her thesis was as follows: 'To the professor, whose name I do not know, who alone voted in favor of my admission to the Ècole, thus protesting against the prejudice that would exclude women from superior studies.' One of the professors on the board took up the dedication, read it aloud to the audience, and then defended himself from the accusations. He 'had never voted, he had no such prejudice, he did not believe that it existed in the faculty, etc., and he considered the claim for right to participate in the superior studies a most legitimate demand.'

"Miss Putnam writes: 'I confess I should not be sorry to have that part of the séance stereotyped for the benefit of New York schools of medicine."

[1] First medical discussion found. *Med. Record*, vol. vi, page 448. Meeting of the Medical Library and Journal Association, October 27, 1871. The topic under discussion: The gymnastic treatment of chorea.

"Doctor Mary C. Putnam, who had closely followed the clinic of the Hôpital des Enfants (rue de Sevres) for the last four years, replied to a question by Doctor Seguin that the gymnastic treatment was still regularly employed there for all choreics able to be out of bed. The milder cases received also sulphuret of potassium baths! Those more severe arsenic! And the worst tartar emetic."

[2] The Medical Library and Journal Association of New York, organized in 1864, to establish a medical reading room and to make the library a nucleus

Association that the young doctor read the first paper presented to a medical society, in the United States, by a medical woman.[1]

Physician. 1871–1902

"Honour a physician with the honour due unto him for the uses which ye may have of him: for the Lord hath created him. The Lord hath created medicines out of the Earth and he that is wise will not abhor them." (*Ecclesiasticus*, Chapter 38.)

The work of a large private practice, attendance at the dispensary and teaching in the college and in the hospital, made what she herself called a "busy day." A private patient said of her "that she was a physician dedicated to the work of helping her fellow-mortals." She was always ready at the moment of greatest necessity.

When the doctor returned from Paris, there were but four hospitals in the United States where a woman was eligible as an attending physician.[2] One dispensary, that of the New York Infirmary, offered her a position on its staff.[3] The Infirmary treated in its little hospital, the first year after the doctor's return from Paris, 144 patients, and this after circulars had been sent in the early part of the season to all clergymen and heads of benevolent societies, stating the nature of the cases received.[4] These patients and those in private practice were studied with the enthusiasm which was a marked characteristic of Dr. Mary Putnam Jacobi. One of the first patients presented an unusual deformity of the heart and this was shown at the New York Path-

---

for a club room. "Stated reunions" were held every Friday evening. "First class papers are read followed by profitable discussions. The Constitutions for 1864 defines those physicians eligible for membership as "any regular and reputable medical man." The Constitution for 1865 omits the word "man" and the doctors Elizabeth and Emily Blackwell were admitted to membership. The Association was merged into the Academy of Medicine.

[1] "Women in Medicine" in *Woman's Work in America*, Mary Putnam Jacobi, M.D.

[2] The New York Infirmary, 1857.
The Woman's Hospital, Philadelphia, 1862.
The New England Hospital, 1863.
The Woman's Hospital, Chicago, 1865.

[3] The students of the college were admitted to certain dispensaries, but only the undergraduates.

[4] Infirmary Report, Jan. 1, 1872.

ological Society, at a meeting, February 14, 1872.[1] The study of pharmacology occupied much of her time. A lecture on one of these early studies was delivered to the students of the college.[2] Lectures on "Medical Botany" were given at the college.[3]

In 1873, Dr. Mary Putnam Jacobi and Dr. Anne A. Angel, a graduate of the Women's Medical College of the New York Infirmary, in 1871, obtained permission from the managers of the Mount Sinai Hospital to attend the children brought to the dispensary. This included all the children under twelve years of age, who applied to the dispensary for medical, surgical or orthopedic aid. Dr. Jacobi continued to attend until 1886. Thus she was responsible for the founding of the pediatric dispensary service at Mount Sinai Hospital, and from 1873 to the present time, a woman physician has been continuously in charge of this clinic. From 1871 to 1897, Dr. Jacobi served the Infirmary as visiting, attending and as consulting physician. From the opening of the hospital, in 1853, until 1886, sick children had been placed in the wards with adults, there being no other place for them. In 1886, Dr. Jacobi opened a little ward containing three beds. The Infirmary report for 1886 notes the fact "that one object for which the Infirmary had been incorporated had been carried out in this year by the opening of a children's ward, completing the name, The Infirmary for Women and Children." In 1891, as a result of her work the ward contained fourteen beds.[4]

From 1893 to 1902, Dr. Jacobi was a visiting physician to St. Marks Hospital.[5]

[1] Anomalous malformation of the heart, *Med. Record*, 1872, vii.

[2] Lecture on atropin, *Med. Record*, 1873, viii.

[3] Report Woman's Medical College of the New York Infirmary, 1872. Mary C. Putnam, M.D., Lectures on Medical Botany.

[4] The New York Infirmary for Women and Children, "Chartered in 1854 as a dispensary, opened with an indoor department, in 1857. From 1857 until 1865, the indoor department of the infirmary was limited to a single ward for poor lying-in women which contained but twelve beds, but in the dispensary several thousand patients a year were treated, and the young physicians living in the hospital also visited the sick poor in their homes. In 1865, a new building was purchased for the hospital, which became enlarged to the capacity of thirty-five beds." *Women in Medicine in America*, by Mary Putnam Jacobi, M.D.

[5] Incorporated, 1890.

Teacher

Woman's Medical College of the New York Infirmary, 1871–1889.

Lectures on materia medica and medical botany, 1871–1872.

Professor of Materia Medica, 1872–1873.

Professor of Materia Medica and Therapeutics, 1873–1889.

New York Post-Graduate Medical School, Clinical Lectures on Diseases of Children, 1882–1885.

"There is no power on earth which setteth up a throne or chair of state in the spirits and souls of men and in their cogitations, imaginations, opinions and beliefs, but knowledge and learning."—BACON.

"After the Blackwells, the most important factor in the movement that brought about the introduction of medical education for women and probably to be considered after them only in time, for her professional influence was co-ordinate with theirs, was Mary Putnam Jacobi." [1] "It was at the time of the greatest difficulty and discouragement for women students and practitioners" when Mary C. Putnam returned from Paris, in 1871. The standards for entering the medical profession were easy of attainment; women "without means or preliminary education could obtain a degree with almost nominal education. It seemed as though the low standard of qualifications then established would prove the most formidable barrier to the success of women in the profession." Mary C. Putnam "brought as her contribution to the new work an enthusiastic love of the scientific side of medicine and a high standard of medical education."[2]

While still a student in Paris the faculty of the little college[3] established by the Doctors Elizabeth and Emily Blackwell had

[1] Walsh. *History of Medicine in New York State*, vol. i, p. 317.

[2] Dr. Emily Blackwell, Mary Putnam Jacobi Memorial Meeting Address.

[3] "In New York, after much hesitation, a charter was obtained in 1865 for the establishment of a medical college in connection with the Infirmary. This step was taken reluctantly because the desire of the Trustees of the Infirmary was not to found another medical school, but to secure the admission of women to the classes for instruction already organized in connection with the medical charities of the city, and to one at least of the New York medical colleges. The demand of women for a medical education had resulted in the founding of small colleges in different places, all, with the exception of the Philadelphia school, limited to the narrow and cheap standard of legal requirements, and producing equally cheap and narrow results in the petty standard of medical education they were establishing among medical women

been waiting for the aid that the accomplished young doctor was to give them. Dr. Putnam had been asked by the Doctors Blackwell to join the faculty and teach materia medica in the new college. At the faculty meeting, May 6, 1870, the Secretary reported "that the return of Dr. Mary Putnam would be delayed by the closing of the University of Paris." The Franco-Prussian War had interrupted her studies. In October, 1871, Dr. Putnam began the lectures on materia medica and in the Spring Session on Medical botany. At the faculty meeting, April 26, 1872, "It was resolved that the faculty recommend

---

students. The establishment of such a school called for money, but the money was forthcoming. A prospectus was issued announcing the requirements. In this prospectus a bold attempt was made to outline a scheme of education which should not only satisfy the conventional existing standard but improve upon this. It was realized, and, oddly enough, for the first time, that the best way to compensate the enormous disadvantages under which women physicians must enter upon their work was to prepare them for it with peculiar thoroughness. Women students were almost universally deficient in preliminary intellectual training; their lesser physical strength rendered a cramming system more often dangerous to health, and more ineffective as a means of preparation; and the prejudices to be encountered in their medical career would subject them not only to just but also to abundant unfair criticism. Instead therefore, of the senseless official system which then everywhere prevailed, it was proposed to establish a three years' graded course, with detailed laboratory work during the first years, and detailed clinical work during the last. A chair of hygiene was established for the first time in America, and an independent Board of Examiners was appointed consisting of professors from the different city schools. By this means, the college voluntarily submitted itself to the external criticism of the highest local authorities. When the Infirmary put forth this prospectus, drawn up by the Doctors Blackwell, no college in the country required such a course. It was deemed Quixotic by many medical friends and several of its features were for a time postponed. The independent board of examiners, however, was established from the beginning, and, little by little, the other parts of the scheme were realized. In 1876, the three year's graded course, at first optional was made obligatory. At this time no college but Harvard had taken this step. The next year the class fell off one-third—a curious commentary on the character or circumstances of the students. In 1881, the college year was lengthened to eight months, thus abandoning the time-honored division of a winter and spring course, the latter comparable to the Catholic works of supererogation, and equally neglected. At the same time entrance examinations were established. These moderate improvements upon the naïve barbarism of existing customs again reduced the classes one-half. When people first began to think of educating women in medicine, a general dread seemed to exist that, if any tests of capacity were applied, all women would be excluded.

to the Board of Trustees to invite Dr. Putnam to continue for another year as lecturer on materia medica with the honorary title of Professor." [1]

At the faculty meeting, September 27, 1872, Mary C. Putnam, M.D., is recorded as present. From that date until her resignation in 1889, her active interest in the college and its students never flagged. It was her constant aim to make the work of her department more comprehensive, more thorough and more useful. She divided her subject into materia medica and therapeutics. The former was taught during the first year and the latter during the two following years. The reasons for the change are eloquently and forcibly set forth in the introductory paragraph of the Lecture on Atropin. The college catalogue for 1873, notes the change.

The interest of the young professor of materia medica and therapeutics was not limited to the problems of her own department. The question which concerned her, and which she studied from every point of view, was the education of women as practitioners of medicine.[2]

The profound skepticism felt about women's abilities was thus as much manifest in the action of the friends to their education as in that of its opponents. But by 1882, the friends dared to "call upon those who believe in the higher education of women, to help to set the highest possible standard for their medical education, and upon those who do not believe in such higher education to help in making such requirements as shall turn aside the incompetent—not by an exercise of arbitrary power, but by a demonstration of incapacity, which is the only logical manly reason for refusing to allow women to pursue an honorable calling in an honorable way. A career is open to women in the medical profession, a career in which they may earn a livelihood; a career in which they may do missionary work among the poor of our own country, and among their own sex in foreign lands; a career that is practical, that is useful, that is scientific. Even when a theoretic demand is not entirely realized in the actual facts of the case, its distinct enunciation remains a great achievement; and, in an almost mysterious way, constantly tends to effect its own ultimate realization, and so it has been here." "Women in Medicine" by Mary Putnam Jacobi, M.D., in *Woman's Work in America.*

The college closed in 1899. The opening of a medical department of Cornell University, admitting women, rendered a separate college for women unnecessary.

[1] Faculty minutes of the Woman's Medical College of the New York Infirmary, May 6, 1870; April 26, 1872.

[2] Faculty minutes, March 7, 1873.

In 1880, the faculty reported favorably upon the request of Dr. Jacobi to hold a weekly quiz for the entering students in anatomy and physiology. This weekly quiz became later "A Physiological Introduction to Therapeutics," given to the first year students Friday afternoons. Thirty-five years later, one of the students remembers vividly this quiz in the old college. "Its scope was not limited to anatomy, to physiology nor to therapeutics, but embraced all related subjects as they were suggested by the question under discussion. Thus, the study of any organ meant a very comprehensive knowledge of its gross and microscopic structure, the source of its nerve and blood supply, its physiology, its correlation with its neighbors in the body. The study of any drug meant the accurate knowledge of its source and preparation and the botanical classification of the plant from which it was derived. Incidental questions of geography, history and literature were also discussed, and ranged through the subjects of medicine, ancient and modern history and the literature of the world. It was the most stimulating course in the first year, full of phenomenal new vistas to a young medical student."

"At the first meeting of this class, Dr. Putnam Jacobi invariably called the roll, looked at each student with a friendly open-hearted glance and asked, 'Where did you obtain your preliminary education?' The doctor knew each student by name after that first session, and on later occasions a stupid answer often brought out the retort, 'As a graduate of —— School, you should know better.' But no matter how stupid nor how uninformed she found her students, she was always patient, friendly and above all, stimulating. The sessions were rarely completed in less than two hours, and by that time there was always a long list of medical questions to be looked into. Incidentally, there were non-medical articles to be read. Never a Friday afternoon that did not stimulate much more than materia medica proper, never a Friday that was dry or dull. The will to learn was what she demanded of her students. She credited them with the mental ability and the industry to do the tasks required. She demanded much, but she gave in overflowing measure, and never spared herself."[1]

[1] Martha Wollstein.

"To her students, Dr. Putnam taught the value of well directed effort for itself alone. No amount of time was too great, no labor too arduous to devote to their interest. She exacted in return care and thought and scientific accuracy. She would not tolerate superficial methods, while for honest intelligent effort, her appreciation was unbounded, and her encouragement, and help always ready. She stimulated others to do the best in their power, and made them realize through her own ideals the greatness of the work which was before them." [1]

The entire problem of women in medicine, especially as practitioners of medicine was still debated. Dr. Jacobi realized fully that every student at the college, as a potential woman physician, must help to make a stronger link in the chain. Therefore she urged that "every student should be really educated and not nominally." The College Catalogue of 1884, she wrote at the request of the faculty, and in it she called attention "to the work in the pharmacological, chemical and histological laboratories," an unusually advanced feature in a medical college of this time.

Dr. Jacobi always held the attention of her audience not only by the content of her lecture or address, but also by her delivery. Her manner was animated; she made no unnecessary gestures nor did she use oratorical methods. Her voice carried well, her face was expressive, illuminated; her eyes large, brown, often twinkling with humor.[2]

A Thursday morning clinic in the diseases of children was given by Dr. Jacobi in the old college, primarily for the second and third year students; others could and did attend. An acute gastric catarrh in its differential diagnosis carried the student through the whole domain of medicine. Every device either in making a diagnosis or in treating the patient was presented.

The necessity for providing adequate medical opportunities for graduate physicians unable to attend the courses given in the universities of Europe had received much attention by the faculties of the medical schools in New York. In this medical problem Dr. Jacobi was greatly interested. The faculty of the Women's Medical College had considered the question, especially in its relation to women. Dr. Jacobi had been appointed in 1880,[3] by the faculty to form a plan for post-graduate instruction in connection with the college and infirmary. The opening of the

[1] Elizabeth M. Cushier's Mary Putnam Jacobi Memorial Meeting Address.

[2] Martha Wollstein, M.D. [3] June 25, 1880, Faculty minutes.

New York Post-Graduate Medical School, admitting women equally with men, made unnecessary any further efforts in this direction by the Woman's Medical College. "In 1882, a school was opened for post-graduate instruction in New York, and Dr. Putnam Jacobi was invited to a place on its faculty, as the clinical lecturer on children's diseases, the first time a lectureship in a masculine school was ever, in this country, filled by a woman." [1] The instruction was clinical, accompanied by charts, maps and microscopical and gross pathological specimens, illustrating the case exhibited. Dr. Jacobi's opening lecture on Diseases of Children was given in a room crowded with men, few women being present. The innovation of clinical teaching necessitated the procuring of patients for presentation. This required the education of dispensary patients. (The Post-Graduate School at that time had neither dispensary nor hospital.) Patients attending clinics controlled by colleges understood that they might be used for demonstration before the students. Dr. Jacobi's patients were sent from her large clinic at the Mount Sinai Hospital dispensary. These patients could not understand and frequently resented being sent to another clinic for this purpose. The most important cases were brought (at times forcibly) by the clinical assistant. Always instructive, the lectures were well attended.

The Association for the Advancement of the Medical Education of Women

Mary Putnam Jacobi, Founder, 1872
President, 1874–1903

Objects: To raise the standard of the medical education of women.

"Article III. For this purpose it shall

I. Create an adequate fund

II. Apply this fund to the proper development of the course of instruction at the Woman's Medical College of the New York Infirmary." [2]

In 1878, "through the influence of the Association, the term of study (in the college) has been extended to three years and the sessions of each year increased to eight months, and preliminary

[1] "Women in Medicine" in *Woman's Work in America.*

[2] Constitution of the Association for the Advancement of the Medical Education of Women, 1874.

examinations are required of students at entrance. The school is the only one in the country, with the exception of Harvard, where these conditions are exacted." "Two additional professorships have been supported, and a library founded." [1] "The assistance of the public is invoked to remedy an injustice which the public has tolerated—that of depriving human beings of the right to educate themselves. Every woman in America who has tried honestly to fit herself for the duties of a physician has been crippled by the organized, almost armed resistance opposed to her efforts to obtain an education." "The real cost of instruction, however, cannot be reduced except by diminishing its real value; for its main expense is that required for the brains of its teachers. At the present day the marked value of intellect is such that the highest instruction cannot be obtained except at an expense far beyond private resources. Intellectual values represent the accumulated wealth of many generations. It is impossible that any single generation should pay for them." [2] The report for 1883 deplores the fact that the association has not succeeded in securing a suitable building for the college. "A building is to a school what a body is to a soul. It may be more imposing than the mental work accomplished and then it is a disadvantage; but it may be so shabby as to depress the spirit of the work and so alienate support from it, and such is our present case." [3] "Health is like the silent existence of those happy nations that have no history. But disease represents the commotion, the storm and stress, the drama and the convulsions into which the disturbed history of our race has usually been thrown." "We aim to exactly supervise the work of every student and to lead each into the knowledge and habit of daily intimate contact with nature, first in health, then in disease."

In a report read at Lakewood, 1884, [4] Dr. Jacobi speaks of some of the students as follows:

"It may interest you to know a few details about some of our students. We have always had a certain number who were studying medicine for the

---

[1] President's Report, 1878.

[2] Our Future Aims, by Mary Putnam Jacobi, M.D. Address delivered at Union League Hall, March 26, 1878.

[3] Report of President, Mary Putnam Jacobi, M.D.

[4] The School of Medicine for Women of the New York Infirmary. Paper read at Lakewood, March 3, 1884.

purpose of becoming missionaries in China and India. The fact that in these countries the women are not allowed to be treated by men physicians at all, offers an obvious field for women. One of our most intelligent graduates has been established for some years in China, has a large practice there, and is at the head of a large hospital. In this she is surgeon as well as physician, and has performed many important operations. This year we have a young Chinese girl as a student—the adopted daughter of an American missionary, who has given her a most careful general education. She is extremely intelligent. The majority of our graduates of course settle down at home, scattered through country towns, rather than in large cities. Several have the largest practice of any of the half-a-dozen physicians in the place. . . .

"While speaking of the students who have made, within our circle at least, some little mark, I must not omit to mention one, who is at present most arduously engaged as physician to the out-practice of the Infirmary. This is composed of the sick who cannot come to the dispensary or be received in the hospital, but must be visited at their own homes. Our hard-working out-door physician receives no salary save her board and lodging; but the energy, fidelity, and conscientiousness with which she attends to her laborious duties could not be purchased for gold. Her district extends from 14th to Houston Street, and from 2d. Avenue to the East River. She often makes, on foot, twenty, or even thirty visits a day to houses scattered far apart through this large district; and does not hesitate to go far down-town if a case presents itself that peculiarly appeals to her interest or sympathies. Where the salaried physicians attached to the city dispensaries would make, grudgingly, one visit a day or less, this girl will not hesitate to return two or three times in the twenty-four hours, if she thinks that the case requires careful watching. She will pass hours in the garrets or cellars of wretched tenement-houses, absorbed in caring for the victims of frequently infectious diseases, and often rendering personal services that the attendants are too ignorant or clumsy to bestow. I shall never forget one case of a little child with diphtheritic croup, upon whom, one midnight last winter, I operated at her request. The child lived in a tenement in Canal Street, more than a mile from the Infirmary. But for ten days the young doctor visited it every two or three hours, and several times passed the entire night by its bedside. With every day that elapsed after the operation, the first faint hope of saving the child's life grew stronger, although in the vast majority of cases such children always die; but the prolongation of life was remarkable, and the young doctor's anxiety and enthusiasm grew constantly more intense. But finally she came to my house early one morning, and burst into tears.

"'The child died last night,' she exclaimed. 'It was dreadful. I wish I were dead too!'

"Perhaps you will call this feminine nerves!

"I might continue to multiply instances of pluck, endurance, intelligence, and heroism from the annals of our institution. But I must not exhaust your patience. And I wish to return to the cardinal point of my subject: the reason, namely, why I bring it at all before you, who are not medical students, nor interested in medicine."

In 1885, the name was changed to The Women's Medical Association, as the name hitherto used is somewhat inconvenient.[1] In May, 1899, the college closed. At a special meeting of the Women's Medical Association of New York City, of which Dr. Mary Putnam Jacobi was president, and at whose home the meeting was held, March 31, 1903, the disposition of the fund unexpended was proposed to be given to the Women's Medical Association. A committee was appointed to arrange the legal conditions necessary for the transference of the money which was accomplished at the annual meeting, May 20, 1903.[2]

Author

"Not he is great who alters matter, but he who alters my state of mind."—*The American Scholar*, EMERSON.

"Books are not absolutely dead things, but do contain a progeny of life in them to be as active as that soul was whose progeny they are; nay, they do preserve as in a vial the purest efficacy and extraction of that living intellect that bred them."—MILTON.

The literary work of Dr. Mary Putnam Jacobi began in her ninth year as stories and essays, expressed in a childish vein, but nearly always in language with a trace of natural eloquence. Her education had been fragmentary. During her earlier years, instruction had been received chiefly from her mother, whose method was to make the little girl read aloud good literature, much reading of nothing but the best and a clear knowledge of the Bible being her substitute for modern training.

Her first published story, entitled "Found and Lost," appeared in the *Atlantic Monthly*, April, 1860, and for it she received eighty dollars. To "supplement her income" while studying in Paris, she wrote for her father's magazine, *Putnam's Monthly*, for the *Atlantic Monthly* and for *Scribner's*. One of these articles, "Some of the French Leaders: The Provisional Government of the Fourth of September," published in August,

[1] Report for 1885.

[2] Minutes of the Women's Medical Association, March 31, 1903; May 20, 1903. The Women's Medical Association was organized when the college closed in 1899, and was composed of the members of the alumnæ association of the Woman's Medical College of the New York Infirmary (organized in 1870) and associate members.

An address delivered before this association will be found on p. 494. The first page has been lost.

1871, is described by Richard Watson Gilder as "one of the ablest ever printed in an American magazine." Thirty-six years later, Mr. Gilder declared,

"I have just been looking again at that article; and bearing in mind all the essays that have appeared in all the magazines which have sprung up, stayed up, and passed down since, and not being sure, either, whether or not, all its conclusions will bear the test of time, I still am inclined to think this same essay is 'one of the ablest ever printed in an American magazine.'" [1]

Eight of these stories and sketches have been collected in a volume. Of these, four show the influence of her medical studies; one, "A Model School," [2] written in 1870, describes what would be considered today (1924) a school in advance of this time in caring for the health and education of babies and children under fourteen years of age. The last essay was written during the siege of Paris, August, 1871.[3]

After her return from Paris, Dr. Putnam's literary work was devoted to medical subjects or to those relating to women in medicine, with the following exceptions: "Physiological Notes on Primary Education and the Study of Language"; "The Value of Life," a reply to Mr. Mallock's essay, "Is Life Worth Living," 1879; "Common Sense Applied to Woman Suffrage," 1894. The first essay relating to women in medicine is entitled, "Shall Women Practice Medicine?" published in the *North American Review*, in Jan., 1882. "Women in Medicine," [4] the most able study yet written of the struggle and final triumph of women in entering the study and practice of medicine. The first paragraph reads:

"The history of the movement for introducing women into the full practice of the medical profession is one of the most interesting of modern times. This movement has already achieved much, and far more than is often supposed, yet the interest lies even less in what has been so far achieved, than in the opposition which has been encountered: in the nature of the opposition; in the pretexts on which it has been sustained, and in the reasonings, more or less disingenuous, by which it has claimed its justification. The history, therefore, is a record not more of fact than of opinion. And the opinions expressed have often been so grave and solid in appearance, yet proved so

---

[1] Mary Putnam Jacobi Memorial Meeting Address, by Richard Watson Gilder.

[2] "Concerning Charlotte."

[3] *Stories and Sketches*, by Mary Putnam Jacobi, 1907.

[4] "Women in Medicine," in *Woman's Work in America*, 1891.

frivolous and empty in view of the subsequent event, that their history is not unworthy of careful considerations among that of other solemn follies of mankind."

Dr. Putnam Jacobi's writings and work for women were mainly devoted to endeavoring to obtain for the qualified woman undergraduate admission to the best medical schools. Largely because of her efforts, Johns Hopkins Medical School opened its doors to women. A world war presented the opportunity to women for admission to any college of their choice, except Harvard, the first to be appealed to in 1847, and in 1924, still closed. In 1891, Dr. Jacobi wrote, "Unless all the opportunities, privileges, honors, and rewards of medical education and the medical profession are as accessible to women as to men, women physicians cannot fail to be regarded as a special and distinctly inferior class of practitioners." [1]

Her first medical writings were letters from Paris to the *Medical Record*, in 1867, entitled "Medical Matters in Paris," signed P. C. M., and were written to "supplement her income." Later ones include lectures, addresses, editorials and scientific papers, some read before societies, two written for the *Cyclopædia of Diseases of Children*, two for Pepper's *System of Medicine*. She was specially interested in the problems of neurology. Hysteria, the subject of several of Dr. Jacobi's papers, has received renewed attention during and since the World War. Of this she wrote in 1886:

"Notwithstanding the voluminous literature which exists on hysteria, something always remains to observe and describe in it. And this is to be expected when it is remembered that hysteria implies disarrangement of the functions of any part of the nervous system in its four spheres of intelligence, mobility, sensibility and visceral neurility. Every advance in our knowledge of these mysterious functions must, therefore, lead to some new point of view in regard to hysteria." [2]

In 1876, Dr. Jacobi wrote the answer to the question, "Do women require mental and bodily rest during menstruation, and to what extent?" For this she was awarded the Boylston Prize of two hundred dollars from Harvard University for the year 1876.

[1] "Open Letters," *The Century Magazine*, February, 1891.

[2] *Essays on Hysteria and Brain Tumor*, and some other cases of nervous disease, 1888.

MEMBER OF MEDICAL SOCIETIES

"Let us be wise, and not impede the soul. Let her work as she will. Let us have one creative energy, one incessant revelation. Let it take what form it will, and let us not bind it by the past to man or woman."

—MARGARET FULLER, 1844.

New York County Medical Society
Medical Library and Journal Association
New York Pathological Society
New York Neurological Society
Therapeutical Society of New York
New York Academy of Medicine
Alumnæ Association of the Woman's Medical College of Pennsylvania
Women's Medical Association of New York City

The story of the struggle of women to obtain official recognition as physicians by admission to the medical societies, Dr. Jacobi relates in *Women in Medicine.* The first application by a woman was made in 1859 to the Philadelphia County Medical Society and was refused. The long struggle ended successfully in that state in 1888. In Massachusetts, in 1860, Dr. Marie E. Zakrewska applied for admission to the State Society and was promptly refused. The struggle was long and bitter, ending in triumph for the women in 1879. In New York State, admission to the County Society met with no opposition. Dr. Emily Blackwell, the first woman, was admitted June 5, 1871; Dr. Mary C. Putnam, November 27, 1871. Dr. Abraham Jacobi tells the story in his Presidential Address of December, 1871.

"Concerning our recent admission, I have another remark to offer. It is not a small satisfaction to me that, in this year of my presidency, one of the most urgent questions of the day should have been quietly and noiselessly answered. The admission of females into the ranks of the medical profession or rather—as their obtaining the degree of M.D. is a matter belonging to

chartering legislatures and their obtaining a practice depends on the choice or prejudice of the public—into the existing medical societies, has been decided by you by a simple vote not attended either by the hisses and clamors of excited young men in medical schools or by the confusion and degradations of the meetings of a medical association. I think we can say that our action has finally settled a question, the importance of which was recognized by everybody. The vote of the largest society of the kind in the Empire State and I believe in the Union will have the effect of soothing the passions and leveling prejudices in the circles of the army of medical men, 40,000 strong in the United States, and of raising us in this respect to the standard of European countries. Even the conservative seat of learning, Edinburgh, has admitted women to medical studies. Paris has turned out a woman doctor of medicine who will prove, I hope, none of the least ornaments of this society, the profession of this city, and our common country."

"Entrée into the New York Academy of Medicine in virtue of special medical work that I have already laid out," thus wrote the enthusiastic Paris student to her mother, January 13, 1870. The desire expressed in this letter was not attained until 1880, when she "was elected, though by the close majority of one to membership in the New York Academy of Medicine, the first woman to be admitted. . . . She was excluded from the Obstetrical Society by means of blackballs, although her paper as candidate was accepted by the committee on membership and she received a majority vote." [1] The first medical society to which Dr. Jacobi was admitted was the Medical Library and Journal Association soon after her return from Paris. It was at a meeting of this society that she for the first time participated in a medical discussion. In 1873, she was admitted to the New York County Medical Society. She was the first woman sent as a delegate from the County Society to the New York State Society, in 1874. Her paper (page 284) was the first presented by a woman at a meeting of the New York State Medical Society. The New York Pathological Society admitted Dr. Jacobi in 1871, and at a meeting on February 14, 1872, she presented her first specimen. Dr. Wyeth, at one time president of the society, writes in 1914, of the meetings.

"Dr. Mary Putnam Jacobi, whose knowledge of pathology was so thorough, whose range of the literature was so wide, and whose criticism was so keen, fearless and just, that in our discussions we felt it prudent to shun the field of speculation and to walk strictly in the path of demonstrated facts." [2]

---

[1] *Women in Medicine*, Dr. Mary Putnam Jacobi.

[2] *With Sabre and Scalpel*, John Allen Wyeth.

"She was a regular attendant at the Neurological Society." "She spoke of the papers read, always with interest, and always with point and brevity . . ." "It was a just recognition of her ambition, that she was finally made chairman of the Section on Neurology of the New York Academy of Medicine." [1]

She also became a member of the Therapeutical Society. She was a member and president of the Alumnæ Association of the Woman's Medical College of Pennsylvania. With the closing of the Woman's Medical College of the New York Infirmary, in 1899, the Alumnæ Association of the college, of which Dr. Jacobi was an honorary member, became the Women's Medical Association of New York. This association she helped to organize and served as president.

Dr. Mary Putnam Jacobi died on June 6, 1906. At the age of ten, she wrote to her grandmother:

"Vague longings beset me. I imagine great things and glorious deeds; but Ah! the vision passes like a fleeting dream and the muddy reality is left behind. I would be great. I would do deeds, so that after I had passed into that world, that region beyond the grave, I should be spoken of with affection, so that I should live again in the hearts of those I have left behind me."

Looking back upon her life and the things that she accomplished, we can only say that she did become great, that she did deeds for which she is spoken of with affection, and that she lives again, and forever, in the hearts of those who knew her.

She has joined "the choir invisible
Of those immortal dead who live again
In minds made better by their presence:
In thoughts sublime that pierce
The night like stars
And with their mild persistence urge
man's search
To vaster issues."

---

[1] Mary Putnam Jacobi Memorial Meeting Address, by Dr. Charles L. Dana.

MARY PUTNAM JACOBI

Dr. Mary Putnam Jacobi began her medical writings in letters from Paris to *The Medical Record*. They ran from 1867 when she was 25 years old to 1871, when she graduated. They gave a very accurate picture of the medical and surgical activities of this time, with occasional personal touches that enlivened her very conscientious descriptive work. These letters still possess interest and have a definite historical value.

When she had established herself in practice in New York she began at once to publish medical articles and she continued to do this yearly in the form of reports, pathological records, addresses and reviews until in 1900 when her final illness overtook her.

At the beginning the subjects of pathology and pathological anatomy especially, interested her. She probably realized that a young doctor with little clinical experience was best fitted to contribute concrete things like pathological specimens about which there would be little question of opinion. She, however, early showed an interest in therapeutics; and this interest was emphasized by her appointment as Professor of Materia Medica and Therapeutics in the Woman's Medical College.

I find in her writings about 50 contributions to pathology; 20 to neurology, about the same number to pediatrics, a dozen contributions to physiology and an equal number to education.

Dr. Jacobi's contributions to medicine were always made with great care. They showed familiarity with previous work and a keen desire to add something definite to human knowledge through her own observations. She quite often brought her special knowledge of social life and of educational problems into her medical investigations. Running through her writings appears her ambition to secure equal rights for women in medicine and in the state, and it is due not a little to her that eventually such rights have been secured.

Dr. Jacobi added many clinical and pathological and educational facts which still remain valuable to her chosen science. She will be remembered by these and also by the example of her courageous and path-breaking career and by her success in promoting the elevation of woman's status as contributors to science and as efficient members of a learned profession.

—Charles Loomis Dana, M.D.

MARY PUTNAM JACOBI

LETTERS TO THE MEDICAL RECORD, 1867—1870—MEDICAL MATTERS IN PARIS. SIGNED P.C.M.

To the Editor of the Medical Record.

SIR—In Paris, the absence of the excitement afforded by political elections is amply compensated, for a limited circle of people at least, by the continually recurring elections at the Academy, and nominations at the *concours*. You are aware that all the hospitals in Paris are under the control of a central administration, who appoint all the physicians. The appointment is made by the decision of a jury, drawn by lot, from among the actual hospital physicians, who decide the merits of the various candidates for a vacancy, after submitting them to severe clinical examinations. A *concours* of this kind has just terminated, in a manner infinitely disappointing to fifty of the candidates who were rejected, and highly agreeable to the two who were deemed worthy for the important position. The successful candidates were MM. Olliver and Praust.

A new contest is now going on at the Academy, which is busy in deciding upon nominations to the chairs of surgery and medicine, left vacant by the death of Jobert and Rostan. Among the foremost candidates for the first position is M. Langier, who has recently added to his previous claims to distinction by an exceedingly interesting memoir upon cerebral concussion. The phenomena occasioned by this accident are analyzed with the greatest care, and referred to that part of the encephalon which, according to present physiological ideas, presides over the functions compromised. The cerebral hemispheres are certainly affected, for the intelligence, all voluntary and affective faculties,

and the consciousness of all sorts of nervous irritation, are entirely suspended. *Unconscious* sensibility, on the contrary, and all movements resulting from reflex action, are, however, preserved, which proves that the pons arolii, and probably the corpora striata and thalami optici are in all their integrity. M. Langier discusses the question, why the hemispheres alone should suffer from a shock that must be transmitted to them through portions of the encephalon that remain uninjured. He accounts for this immunity on the part of the pons, the bulb, etc., by the fact of their superior firmness of structure, and the more secure position of their gray masses, which, being in the centre of the white tissue, are much less exposed to shock than the gray substance of the hemispheres distributed over their surface.

It is not certain whether this memoir will elect M. Langier, but it is discussed in his favor. Another illustration of the value of disease in dissecting apart the involved functions of the brain, is furnished us this week by a most interesting case at the Hôpital St. Antoine, in the service of M. Jaccaud. It was a case of aphasia, the disease that has been rendered so famous by M. Broca's theory, which attaches it to a lesion of the third anterior convolution of the *left* cerebral hemisphere. The accident is not uncommon, but the opportunity for verifying the diagnosis by an autopsy is comparatively rare, as the disease is rarely or never directly fatal. Hence this case, which afforded such an opportunity, is one specially valuable.

The patient in question was already a victim to Bright's disease of the kidney, for which he had been in the hospital since last June. At the period of the accident he suffered from an extensive œdema, without ascites; albumen was abundant in the urine; he presented, moreover, a systolic souffle at the point of the heart. On the 22d of January, without any premonitory symptoms, this patient suddenly discovered that he had lost the power of speech, and on the morning visit, the next day, he was found in a state of great disquiet, pointing to his lips and tongue, indicating by signs that he wished to speak but could not. There was not the slightest lesion of any of the limbs, and at the face only a slight paralysis of the zygomatic and elevator muscles of the right side of the mouth, which was drawn a little to the left.

The understanding was not in the least impaired; he took up any article that was named to him, but could not name them

himself. When a person pronounced before him a word distinctly articulated, he examined carefully the motion of the lips, and succeeded in uttering some monosyllable, as "bien," "vous," but that was all. He was equally unable to write as to speak, never getting beyond the first letters of his name.

In presence of these symptoms, and on account of the mitral insufficiency, M. Jaccaud pronounced a diagnosis of lesion of the third frontal convolution, probably at the left (since that is the case seven times out of ten), and probably in consequence of an embolus.

The aphasia began to disappear by the end of the 30th, and by the end of February was entirely gone. The patient died on the 22d of April, and at the autopsy the diagnosis was fully confirmed by the discovery of—first, complete fatty degeneration of the kidneys; second, vegetations on the mitral valve, and insufficiency; third, buried in the white substance of the third frontal convolution of the left hemisphere were two hæmorrhagic cysts, which contained some drops of liquid. One of these cysts was the size of a pea, and situated at the right of second, whose volume was three times as considerable. The surface of the section was distinct and well limited; not only the lesion did not extend beyond the convolution in question, but the gray matter of that was untouched, and from the exterior appeared perfectly healthy. The other parts of the encephalon, as also the membranes and the arteries, were examined with great care, and found perfectly sound.

Hence this case brings fresh support to the theory that places the power, not merely of speech, but also expression by writing, in this limited part of the brain.

Among the candidates for the vacant place in medicine at the Academy, M. Hérard is one of the most prominent. His claims to the honor chiefly rest upon a work that he has published this year upon pulmonary consumption. The initial idea of this striking book was originally promulgated in Germany by Reinhardt, but M. Hérard and his colleague, Cornil, have done much, not merely to popularize Reinhardt's views, but to bring to their support abundant clinical demonstration. You are aware that Reinhardt completely upsets the old descriptions given by Laennec and Louis of the cheesy tubercle. According to recent microscopical researches, the yellow masses thus denominated

are formed, not by the ulterior development of a heterologous deposit, but of a pneumonia, excited by the presence of the crude tubercles in the connective tissue of the lung. This pneumonia differs from ordinary acute pneumonia, rather in its anatomic and microscopic characters, than in its constitutional effects. Instead of an exudation of fibrine into the alveoles, there is an exudation of pavement epithelium and leucocytes. In other words, it is a *catarrhal* pneumonia, similar with that caused by artificial experiment. To this pneumonia Hérard and Cornil refer all the general symptoms of phthisis, the fever, emaciation, and destruction of the vital forces. They maintain that the presence of the crude tubercle in the lung excites *no* general disorder, and it may remain latent for an indefinite period, until, generally in consequence of some special accident, the tissue surrounding it inflames. This inflammation may subside spontaneously or under the influence of treatment, but relighted again and again, it finishes by entering upon the cheesy state (*pneumone caseuse*). At this stage the contents of the alveoles liquify, the hepatised lung softens, and the cavern is formed which for so long has been exclusively attributed to the softening of the tubercles. These also soften, but their size always remains the same as that of the original gray granulation. Even when softened they may be distinguished from the masses of pneumonic lung through which they are disseminated, by the presence of small nuclei and the cellules, called by Robin cytoplastians.

In consequence of this view of the anatomy of phthisis (which approaches in its nature somewhat to that proclaimed by Broussais), M. Hérard lays special stress upon local revulsives in the treatment, iodic frictions, blisters, and the actual cautery. The tonic and stimulant general treatment is of course also maintained, though our American use of alcohol is rejected. To a certain extent, a moderate use of tartar emetic, as recommended by Foussagrines, is counselled.

This treatment does not procure brilliant results in the hospitals, where the patients generally are too far advanced to be saved, but M. Hérard declares that a fair share of success may be obtained in private practice, where the disease is treated at the very beginning.

M. Marriothe communicates to the *Bulletin de Thérapeutique*, an account of some clinical experiments made by himself this

winter with muriate of ammonia, in the treatment of catarrhal fevers that have been epidemic in Paris. These fevers assumed a remittent or intermittent type, without losing their distinctively catarrhal character, but proved quite obstinate to sulphate of quinine. In accordance with a suggestion by Schmidtmann, who was in the habit of giving muriate of ammonia in the declining period of gastric fevers when they assumed a periodical form, M. Marriothe tried the experiment, at first in some cases complicated with very severe neuralgies, which interrupted sleep, and even extorted cries from the patient. The effect of the muriate was surprising. In mild cases the febrile attacks and the neuralgies ceased upon the first or second day; in more severe forms the success was not complete till the third or fourth, but there was always amelioration by the first or second.

M. Marriothe thinks that the salt has an important influence in moderating the erethism of the mucous membranes, but that, besides, it acts directly on the nervous system, without the occurrence of any intermediate phenomena, as vomiting, sweating, diarrhœa, etc. The dose found necessary to arrest the febrile attacks and calm the neuralgia, varied from thirty to sixty grains in the course of the day, being administered in portions of 7–15 ʒ every three or four hours.

An operation that Dr. Brown has done much to bring into favorable notice in England, is beginning again to excite the attention of French physicians. I mean the capital operation of ovariotomy, upon which M. Boinet has just read an elaborate report before the Academy. The report begins with reference to an American, the first who practised ovariotomy with the definite intention of extirpating the diseased organs, Dr. Ephraim MacDowell, of Kentucky. This surgeon, between 1809 and 1830, operated thirteen times, and obtained eight cures. Baker Brown reports twenty-nine successful cases out of thirty-two operations. Even were the success in much smaller proportion than this (and the recent exposé of Brown's character renders us cautious about accepting his statistics), the operation would be legitimate in a disease that conducts its victims almost inevitably to the grave, the deaths being 95 in 100. M. Boinet reprobates the timidity of the French surgeons, who have so long recoiled before this operation, and proceeds to give many useful hints upon the precautions necessary to insure success.

In the first place, the operation should never be performed at a hospital, where peritonitis invariably follows the opening of the abdomen; a healthy, isolated locality should be selected, and a room prepared whose temperature should be maintained at 20 to 25 degrees centigrade. Secondly, the nature of the cyst must be carefully considered. If it be simple, unilocular, containing a liquid, clear, limpid, and serous, or even purulent or sanguinolent, the operation is inappropriate. Nelaton's system of iodine injections should be first tried. But if the liquid, though at first serous becomes unctuous and fatty, ovariotomy is the only resource. Other proofs that the cyst is multilocular, or that it contains pathological productions contra-indicating the use of iodine, signs of the increase of the tumor, and exhaustion of the patient, are circumstances that should also call for the operation. Among the contra-indications should be reckoned the existence of grave complications, an early stage of the disease, pregnancy, or tumors in the uterine walls.

The operator should place himself at the right or left side of the patient, instead of between the knees, as recommended by some surgeons. The incision should be made on the median line, and of sufficient size to admit the free introduction of the hand in the cavity, for the purpose of recognizing the size and position of the tumor and the extent of the adhesions, which exist three times out of four. When these adhesions are slight and can be easily torn, they occasion no inconvenience; but if large and resistant, their action frequently causes dangerous hæmorrhage. The dangers of too short an incision have been frequently exposed; the adhesions are not distinctly perceived, the cyst is imperfectly grasped, the other ovary cannot be seen, the pedicle is tied with difficulty, and sanguineous effusions may take place in the abdominal cavity without the knowledge of the surgeon. Should the first incision prove too short to avoid these inconveniences, a second can always be practised with safety.

The incisions should divide the different layers of the abdominal wall in succession, to avoid too sudden entrance into the cyst. As soon as this is discovered, the hand should be introduced, to ascertain the existence of adhesions or neighboring tumors. In the second case, it is only necessary to enlarge the incision; in the first, the adhesions must be detached by the hand, or destroyed by the scissors or hot iron. They should not be torn.

Before tapping the cyst two assistants should press upon the abdominal walls, in order to force the cyst to project between the lips of the incision. The puncture is then made with a trocar, and by means of the foregoing precaution the liquid is prevented from running into the peritoneal cavity. If there is more than one pouch, the first should be held by pincers, or tied, while the others are drawn out of the cavity. If the cyst, on its retreat, draws a portion of its adherent intestine, it is important that this be detached; if that be impossible, a piece of the cyst must be cut cut, and left attached to the intestine. Care must be taken, however, to remove the internal secreting membrane from this fragment.

All bleeding vessels, whose volume is not too considerable, should be twisted, or cauterized with a hot iron or the perchloride. Only when this method is impossible should the vessels be tied.

The wound should not be closed until the last drop of liquid has ceased to flow. An eminently useful precaution consists in placing in the inferior angle of the wound, or in the recto-vaginal cul-de-sac, a caoutchouc tube, by which any liquid subsequently effused may drain off. MM. Keith and Koeherle attribute a great number of their successes to the observance of this precaution. Boinet has no dread of the introduction of air into the peritoneum, attributing all inflammation to the action of liquids, and not air.

A double line of sutures is necessary, one deep, the other superficial. M. Boinet decides in favor of including the peritoneum in the suture, to avoid the danger of this membrane contracting adhesions with the intestine. The pedicle should be compressed by a toothed clamp, which has the advantage of compressing the tissues and preventing hæmorrhages. If the pedicle is voluminous, it should, however, be tied, or the ligature combined with the clamp. When the size of the pedicle, or its insertion on the uterus, presents unusual difficulties, a thread of ligature should be passed around it before the cyst is cut away.

M. Boinet concludes,

"that the ovariotomy should be accepted with as much enthusiasm as all other capital operations; and that now that the bases of diagnosis are better assured, and the operative procedures more perfect, the subsequent treatment better understood, more advantageous results will doubtless be obtained."

M. Demarquay, surgeon of a Maison de Santé, presents a report on the topical application of iodoform in the treatment of

cancer of the uterus. This agent employed in the crystalline form, in a dose of 7 to 15 grains, is mixed with a sufficient quantity of butter of cocoa, and the suppository thus formed, introduced intò the vagina, or an ulceration of the carcinoma, if that exists,—a tampon of cotton is placed in front. The general effects are slightly observable, although the iodine from the drug (C^2HI^3) is absorbed and may be discovered in the saliva and urine. But the local suffering is almost infallibly soothed; the swelling of the abdomen diminishes; and the ease procured lasts as long as the medicament is continued, is broken up by the interruption of its administration, and reëstablished when that is renewed.

Dr. Morel, the distinguished alienist, writes an interesting article in the Archives of Medicine for May, upon progression in hereditary insanity and nervous diseases. Not content with the vulgar fact of hereditary influence in the transmission of such disorders, he, in company with an army of modern *confrères*, seeks the laws that govern this hereditary transmission. I have not space left to enter into all the details of his curious paper, I can only mention three or four of the most striking conclusions, drawn from a great number of facts: 1st. Insanity, epilepsy, hysteria, chorea, eccentricities, dypsomania, etc., are only the branches of an identical constitutional vice of the nervous system, and may be transformed, the one into the other, by way of hereditary transmission. 2d. Such transformation is more frequent than the transmission of the same form of disorder. When a simple eccentricity of a parent becomes insanity in a child, &c., the hereditary taint is said to be progressive ascendant, and the opposite case progressive descendant. 3d. Whenever the several children of parents presenting a nervous taint, are *markedly dissimilar* in appearance and character, the taint will almost surely be transmitted, and progressive ascendant. It seems in this case as if the whole force of family likeness was concentrated in the depths of the nervous system. 4th. In such families it is common to observe that one or more of the members are gifted with remarkable intellectual ability, while others are idiots.

The third proposition especially constitutes the theme of M. Morel's present paper. The fourth identifies his views with those of Dr. Moreau, who, in his remarkable work on morbid psychology, unhesitatingly ranks genius among the neuroses,

and assigns to it an origin identical with that of epilepsy, insanity, and idiocy.

PARIS, June 18, 1867.

P. C. M.

To the Editor of the Medical Record.

SIR—At the last *séance* of the Academy of Medicine the discussion on tracheotomy was continued, and M. Peter made quite a discourse on the subject, describing the practical difficulties in the way of the operation, and suggesting means of overcoming them. Among the principal, is the small size of the trachea in young children. The operator is liable, in making an incision with the bistoury directed perpendicularly to the windpipe, to pass completely through that organ to the œsophagus. Again, the index-finger used to hold the trachea in place, frequently pushes it to one side, so that the incision is made to the right or left of the median line. This would be of small consequence if the trachea was steadily maintained in the first position, but too often the finger slips, the trachea returns to the middle of the throat, and the incision is concealed from view. These difficulties are more formidable as the child is younger, but M. Peter thinks that the age alone never offers a formal contra-indication, since it is well in case of need to operate on the smallest child, and give it a chance for life at a moment that all others are lost.

M. Peter considers, however, that tracheotomy is never necessary, and therefore never advisable, except in cases of pseudo-membranous croup, and believing that pseudo-membranous angina invariably accompanies this disease, he abstains from the operation whenever he cannot find false membranes in the pharynx. The extension of the membranes to the bronchial tubes is, however, of course a circumstance of unfavorable omen for the operation. It is often difficult to diagnose this complication. M. Moutard Martin signalizes 'pale asphyxia as an excellent sign. M. Peter adds, an unusual frequence of the respiratory movements, whose rapidity is slackened in simple laryngeal croup. Whenever there are more than fifty inspirations a minute, there is good reason to suspect a pseudo-membranous bronchitis.

Subsequent to the operation, the practitioner possesses another sign of this formidable complication. When the canula left in the tracheal wound does not become filled with mucosities, when at the end of twelve hours it is still dry, there is too good reason to believe that the mucous surface of the bronchial tubes is covered with false membranes which effectually prevent secretion.

M. Peter declares that pseudo-membranous bronchitis is quite frequent, occurring, in his experience, 52 times in 105 fatal cases.

Pneumonia coincident with the croup does not absolutely forbid the operation. M. Grisolle observes, that his first successful case of tracheotomy was embarrassed by this complication. M. Nelaton had been called upon to operate, but recognizing the concomitant pulmonary lesion, refused. M. Grisolle then assumed all the responsibility, and operated himself—the child recovered.

M. Archambaud had obtained 21 cures among 67 operations, including two upon adults. The most of these cases had already reached the last period when the operation was performed, the patients sometimes being completely insensible. Among 53 operations made under such circumstances, 17 had succeeded, and among 12 cases treated at an earlier stage of the disease, 4 were saved. The proportion therefore is about the same.

M. Peter assumes as contra-indications, an excessive waxy pallor, ganglionic engorgement, extreme puffiness of the neck, which is neither œdema or emphysema, all signs of general intoxication.

Cases of stridulous laryngitis are successfully treated by M. Peter with steam. The child is surrounded by half-a-dozen basins filled with boiling water, so that the respiratory organs may be incessantly bathed in the humid atmosphere; the croupal symptoms generally subside in about an hour.

The month of June has been unfavorable for the performance of tracheotomy, since out of eight operations, divided equally between the Children's Hospital and Hôpital St. Eugénie, six have proved fatal.

Appreciation of Medical Constitutions.

This observation enters into a report, presented by Mr. Besnier to the Academy of Medicine, on the medical constitution

for June. An effort is being made just now to collect materials for a rigorous appreciation of "medical constitutions." The value of such an appreciation cannot be too highly estimated, when it is remembered how largely this condition enters as an element into the effect of medical treatment. There can be no question, that the reputation of a large number of methods and medicines has been made by the fact that they were administered at a moment when the disease had assumed a benign type, and tended of its own accord to a favorable issue. During this month of June, nearly all the cases of typhoid fever in the hospitals have recovered. There has not been less, but rather more of the disease than usual, and the first stages have frequently opened with considerable severity, but any dangerous symptoms have quickly abated, and the course of the malady has been equally satisfactory under any treatment. This reminds me of an amusing anecdote related by Dr. Maximin Legrand in the feuilleton of the *Union Médicale:*

> One day, the *garçon de service*, employed in the wards of M. Fouquier, appeared with two black eyes, and his face covered with bruises. "What is the matter with you, my man? "inquired M. Fouquier, always kind and polite. "I have been fighting with M. Bouillaud's *infirmier*, and he is better done for than I am." "You were very wrong; what were you fighting about?" "Because he insisted that it is always necessary to bleed in typhoid fever!" The gravity of the physician was not proof against this unexpected reply.

When it is remembered that M. Bouillaud is the author of the famous system of bleeding in pneumonia twice a day, *coup sur coup*, and extends his sanguinary propensities to typhoid fever also, the belligerent enthusiasm of his humble subordinate may be easily explained.

The Administration of Mercury in Syphilis.

At the Imperial Society of Surgeons, the discussions still turn upon the question of the administration of mercury in syphilis, a question that seems subject to periodical agitation. The most conspicuous part of the debate has been that sustained by M. Desprès whose views have been entirely special, "so special," observes Dr. Reveillant, "that he remained entirely alone in his opinion." For M. Desprès is radical enough to deny any efficacy to mercury whatever, in the disease in which it has for so long been considered the sheet-anchor. He is resolved

never to administer the baneful drug either in primary, or secondary, or tertiary syphilis. Theoretically, he bases his principles upon the idea, that the malady is already so exhausting to the patient, that the debilitating effects of mercury can work him nothing but injury. Practically M. Desprès appeals to the result of his experience in the Hôpital Leourcine, among 234 patients, of whom some were subjected to the classical treatment, others to a course of tonics. Among the first, a percentage of 28 for 100 returned after more or less time to the hospital for fresh treatment of the disease, while in the second class the returns were only 10 for a hundred. M. Desprès declares, rather fancifully, that the physician should endeavor to restore his patient to a "life of infancy," regulating his food, sleep, and exercise, building up his shattered constitution, and that nature would eliminate the poison.

"We cannot believe that a purely empirical medicine can be a contra-poison against syphilis, or that there is any sense in employing a drug that exists in the blood like foreign matter, which does not assimilate with a single fluid, and which, even in exercising a certain perturbating and deleterious effect on the economy, neither solicits nor suspends the regular exercise of a single function."

The only points of importance in M. Desprès' remarks, are the statistics, and their value is vigorously contested by M. Depaul. He observes that the comparison between the two modes of treatment was not sufficiently extended, and moreover, that the basis of comparison was fallacious, since a number of the patients who had been treated by expectation, probably absented themselves from the clinique, not because they were cured, but because they were disgusted with the treatment. Nothing exasperates a hospital patient so much as the suspicion that nothing is being done for him. He maintained that this expectant system was extremely dangerous, since the most serious destruction of tissue, such as the perforation of the palatine vault, might take place while the physician was watching with folded hands. M. Depaul laid especial stress on the efficacy of mercury administered for syphilis contracted during pregnancy, when, he declares, it *uniformly* prevents abortion, in recent cases.

A case that recently occurred in the service of M. Hérard at Laribaissière, which I had an opportunity of observing myself,

is in entire accordance with this assertion. The subject was a woman of about 35, in the third month of pregnancy. Several years previous she had been treated at Lourcine for primary syphilis, and two years ago had been an inmate of Laribaissière for syphilitic angina. Each time she had completely recovered, and it was impossible to ascertain whether the renewal of the disease was a manifestation of the original malady, or the result of a new infection. At the moment of her entrance, in June, the patient presented an eruption of syphilitic erythema, copper-colored blotches disseminated over the entire body; a small tumor on the right frontal bosse, with broad base, but slight elevation, but the seat of lancinating pains exasperated at night, and which extended also to the temples and the ears: a gray ulcerated fissure at the left commissure of the lips; a grayish *plaque muqueux* on the right labium majus of the vulva. Fine subcrepitant râles could be heard at the summit of the left lung. A cough had existed for several months, but the patient professed to have been perfectly free from syphilitic accidents at the commencement of her pregnancy. She was ordered a pill of corrosive sublimate containing five centigrammes, to be taken every evening. This was the 11th of June. On the 12th, in addition she commenced to take 25 centigrammes of iodide of potassium every morning. By the 19th the pains in the head had entirely ceased, and the eruption had begun to fade. By July 11th the tumor had almost disappeared, as also the *plaque muqueux*, and the eruption was entirely gone. The fissure of the lips was also healed, and the patient left the hospital on the 16th of July in a perfectly satisfactory condition (the cough also was diminished), both as regards her general health and the march of the pregnancy. The treatment was continued uninterruptedly during the first month; after that, the sublimate was suppressed, and the iodide alone continued.

MM. Guérin, Perrin, Verneuil, and Velpeau, also took up arms in defence of mercury. They nearly all insisted upon a *prolonged* treatment, not less than two years, as absolutely necessary to radical cures. M. Guérin therefore disapproves of large doses. He prefers the protiodide associated with opium, but in case that is supported with difficulty, he has recourse to fumigations with cinnabar. He does not believe that inunctions alone are sufficient, while they have the inconvenience of pro-

ducing salivation more speedily than other methods of administration. M. Perrin, with an experience of 470 cases, treated at Val de Grace, disbelieves that mercury administered in primary syphilis can prevent the regular evolution of the disease, and therefore confines himself to local cauterizations, and only commences general treatment with the appearance of secondary symptoms. He is careful to administer chlorate of potassa coincidently with the mercury in any form. He acknowledges that no treatment is infallible against relapses, but that the physician is simply called upon to be persevering, and reapply the treatment at each outbreak until he has mastered the disease.

M. Verneuil and Velpeau believe in the beneficial effects of mercury at all periods of the disease, and the former declares that salivation is an imaginary phantom. Syphilis *may* exhaust itself spontaneously, but such cases are rare, and generally the patients are exhausted first.

The only person who in any way sustained the views of Desprès was M. St. Germain, who, while professing to believe that mercury does render some indefinite service in syphilis, declares at the same time that he considers it as useless against chancre, that he has a "certain tendency not to administer it in secondary syphilis," and that he always combats tertiary symptoms by iodide of potassium.

It appears, therefore, that not much new light has yet been thrown on this important subject by the debate. The society is waiting to hear the opinion of M. Diday, pupil of Ricord, whose voice would naturally have much influence.

More original views were presented in a recent discussion at Lyons, on the same subject, where several of the members maintained that it was unnecessary to spend much time or thought upon the cure of patients who had fallen a prey to the disease in consequence of misconduct, and that especially such patients should never receive the benefit of gratuitous treatment.

Vesico-Vaginal Fistula Treated by the American Method.

M. Courty, Professor at Montpellier, publishes an account of six cases of vesico-vaginal fistula, successfully operated by the "American method." The *Bulletin of Therapeutics* publishes the details of two that presented unusual difficulties. In one, the fistula, five centimetres long, dated from four years, the

urethra was obliterated, and vagino-pubic adhesions existed. There was also a hernia of the bladder. Owing to the adhesions, the operation was exceedingly difficult, the hæmorrhage abundant, and the lips of the wound did not completely close. After a second operation, however, performed upon the gaping part of the suture, adhesion was effected, and a radical cure completed. The treatment lasted four months. In the second case, the patient, a woman 28 years old, had already been operated upon unsuccessfully, and a cicatricial tissue, hard and thick, bordered the edges of the fistula, which was eight millimetres long. In this case also, two operations were necessary.

M. Courty only revives the borders of the fistula, at the expense of the vaginal mucous membrane, carefully avoiding the vesical. For the deep sutures he uses Startin's needles, and for the superficial, Sims'. He leaves a sound constantly in the urethra, and by means of a canula pushed to the bottom of the vagina, has the cavity washed out twice a day with a lotion to prevent suppuration. The wires are withdrawn between the fifth and tenth days.

Mistake between an Ovarian and Renal Cyst.

The *Gazette Hebdomadaire* quotes a case of a mistake made between an ovarian and renal cyst by the distinguished Dr. Wells of London. A woman of 43 years presented herself at his hospital, to be treated for an abdominal tumor, that two experienced physicians had already pronounced to be an ovarian cyst. They had refused to operate, however, because a loop of intestine was recognized as passing in front of the tumor. On the 4th of August, when Mr. Wells first saw the patient, the tumor had risen to the epigastrium, and the patient seemed threatened with suffocation. He punctured the cyst to her immediate relief, and the tumor and dull percussion sound entirely disappeared. Two months later, the woman returned to the hospital, with the tumor again filling all the abdomen. On the left of the umbilicus was recognized a hard band, which was supposed by some to be a loop of intestine, by others the Fallopian tube. The menstruation was regular; the urine contained mucus and epithelium, but no albumen. The abdomen was largely opened in the median line. The incision of the peritoneum revealed passing in front of the cyst, the transverse and descending colon,

intimately adhering to the abdominal walls and also to the cyst; 15 pints of gray purulent liquid were withdrawn from the cavity. The destruction of the adhesions revealed a second cyst, which yielded two pints of clear liquid. Finally, since it was impossible to destroy the deep adhesions, the cyst was left in place and the wound closed. The patient succumbed the next morning. At the autopsy the following state of things was manifest: Four pints of sanguinolent serum and of coagula were effused in the peritoneum. *The uterus and ovaries were perfectly healthy.* The right kidney was hypertrophied, and much softened, a calculus of 40 centigrammes was found in the calix. The left kidney was converted into a cyst more voluminous than the head of a fœtus, containing a single cavity divided by bridles, and whose walls were formed by the renal capsule. The parenchyma had completely degenerated and atrophied.

A precisely analogous case occurred here the other day, at La Pitié, in the ward of M. Belrier. The patient was 48 years old, and feeble, and on this account it was decided that ovariotomy was unadvisable, although an ovarian cyst was diagnosed without any hesitation. Tapping was followed by the complete collapse of the tumor, which, however, resumed its original dimensions in two or three weeks. At the time of the patient's death, two or three weeks after the operation, the cyst contained several quarts of liquid, and occupied all of one side of the abdomen, from the iliac fossa to the hypochondrium. At the post-mortem, this tumor was found to be an enormous cyst of the kidney, whose entire parenchyma was destroyed, and only the capsule left, lined by a serous membrane of new formation, but of sufficient secreting power to reproduce the entire volume of liquid in the course of two or three weeks.

Mr. Wells has profited by his mistake to make a more careful study of the points of diagnosis between renal and ovarian cysts, and has published some most valuable reflections. The diagnosis, he says, should be based on the following circumstances:

1st. Whenever a bridle of intestine is recognized as passing in front of the cyst, it is almost certainly renal, since the ovarian cysts push the entire intestinal mass of it against the vertebral column.

2nd. The ascending colon would be found on the internal side of the right kidney; the left is crossed from above downwards by the descending.

3rd. The urine, which should be subjected to a microscopic examination, nearly always contains mucus, epithelium, pus or albumen, in cases of renal tumors, whilst the menstruation is not disturbed as it is in ovarian disease. (This latter circumstance evidently cannot be relied upon in the numerous instances where the tumor is developed after the ménopause.)

4th. The bridle, on percussion, is found to be contracted like a cord, and is mobile.

5th. In the case of ovarian cysts, the liquid often escapes by the Fallopian tube, after adhesions have been contracted, while in renal cysts the way of escape is by the ureter and bladder.

6th. Renal tumors appear first in the hypochondria, and develop downwards; ovarian in the iliac fossa, and pass upwards.

Mr. Wells concludes that henceforth no one need make a mistake between the two diseases.

Cancer of the Kidney, etc.

While speaking about renal tumors, I must mention a highly interesting case at present in the service of M. Hérard, at Laribaissière, where an inverse mistake in the diagnosis was induced by the ambiguity of the symptoms. The subject, a woman of about 38, entered the ward the 31st of March, presenting an abdominal tumor that occupied the left iliac fossa, and extended in front to the umbilicus, and behind to the spine. A smaller tumor was situated in a precisely similar manner at the right side. The patient had begun to suffer five months before her entrance, with severe pains in the renal and dorsal regions, which were presently followed by the development of the left tumor, which rapidly increased to its present size. The disease had attacked the right side about a month ago. The anterior border of the tumor could be felt distinctly in front; behind the limits were more vague. Clear percussion sound was obtained between the dulness of the tumor, and that proper to the spleen. Also, the mass did not continue into the inguinal region, or pass the median line, so that the idea of an ovarian disease was set aside. The position at the left of the principal mass put the liver out of the question, and the appearance of a similar tumor in the right renal region indicated that a symmetrical organ was invaded. Everything, therefore, led to the belief that the disease occupied the kidney, a belief (as I hasten to say) that so far

nothing has contradicted. But the tumor presented quite distinct fluctuation. The complexion of the patient, though pale and chalky, had no tint of special cachexia, and the diagnosis of renal dropsy (*hydronephrose*) was pronounced. A surgeon in consultation agreed in this opinion, and tapped the tumor. There issued, neither urine nor serous fluid, but a small quantity of juice, which, both to the naked eye and the microscope, was evidently cancerous. The greater part of the tumor was, after all, solid.

The patient is still alive, and her condition is liable to great variations. For a long time after her entrance to the hospital she suffered almost continually from pain, which, finally, seemed to be relieved by subcutaneous injections of morphine. A week ago she was a great deal better, sat up, embroidered, felt quite at her ease, but a relapse has just occurred, and she is now about in the same state as when she entered. During the first weeks the tumor seemed to increase, but for the last six weeks it has been quite stationary. Since the tapping (which did not materially diminish the size of the tumor), the sensation of fluctuation has disappeared, and now the surface of the mass is more uneven, though never hard, or distinctly bosselated. The cancer is evidently an encephaloma.

Contractibility of Muscular Fibre.

Before closing my chronicle, I must tell you of some singular experiments that have just been made by M. Rouget upon the contractibility of muscular fibre. M. Rouget commenced his researches on the subject, by the study of the style of the vorticellus, where the muscle consists of a single fibre. This is elongated during life, but under the influence of excitants, or after the death of the animal, the spiral returns brusquely on itself, and is shortened four-fifths, being transformed into a spiral spring, pressed closely together. Experimenting subsequently upon living animals, Rouget found that everything that interfered with the nutrition of the muscles, made them contract. If the main artery of a limb were tied, if galvanic excitement was continued incessantly, if the muscles were subjected to a continually increasing heat or to cold, the result was always the same, *they contracted*. When the contractions were too frequent, the myographion showed that the transverse lines repeatedly approached each other, could no longer separate, but remained,

as it were, agglutinated. Rouget declares that the primitive muscular fibre is constituted by an elastic fibre twisted in a spiral, and that the transverse lines mark the curves of this spiral, and not the segmentations of a straight bundle of fibrillar elements, as usually maintained. The state of repose, the normal state of this spiral, is that of the approximation of its rings, which appears to the eye as the contraction of the muscle. The lengthening is the really active process, and can only occur during the vigor of life. The cadaveric rigidity of muscles is precisely the same phenomenon as that occurring when their vitality has been exhausted by heat or cold, or starved out by lack of food. When a muscular fibre shortens, it does so in virtue of its own elasticity, which triumphs over the vital force developed in the act of nutrition. This or any other force that excites motion in the muscles, at the moment that it ceases to act, is transformed into heat, and hence the rise of temperature observed in muscles entering into a state of contraction.

Muscles do not contract in successive undulations or shocks, except at the beginning of the action of an external excitant, or when they are exhausted by fatigue. Contracted muscles seen under a microscope, are found to be perfectly motionless. When they contract by the will, there are no undulations even at the beginning of the period.

The influence of this theory, which reverses the passive and active sides of muscular movement, upon tetanus, chorea, and all diseases of muscular activity, is easily perceived. But M. Rouget as yet attempts no pathological applications.

Un joli mot, as the French say, in conclusion. You are familiar with the name of Charcot, I suppose, and of his intimacy with the distinguished surgeon, Vulpian. The two have so often published together, that their names are inextricably associated to the public ear. The other day a friend of Charcot's observed:

"Charcot has been made happy this morning. He is the father of a son."

"What," exclaimed a bystander, "Charcot and Vulpian?" But it was explained that this time it was Charcot, *tout seul*.

The Origin of Modern Anæsthesia.

I have not yet finished, for I must mention the compliment paid by the *Gazette Hebdomadaire* to the MEDICAL RECORD, as

"the most serious medical journal in the United States." When the RECORD ascribes the first (chronological) honor of chloroform to Dr. Wells, the *Gazette* thinks that the question is settled.

P. C. M.

PARIS, August 19, 1867.

To the Editor of the Medical Record.

SIR—The School of Medicine held its annual closing ceremonies on the 14th. M. Béhier pronounced an eloquent eulogy upon Rostan. On the 17th the amphitheatre of the Ecole was again filled to celebrate the opening of the International Medical Congress, where seven hundred physicians from all parts of the world, representing nearly all the celebrated physicians living, had gathered together.

The International Medical Congress.

The hemicircle was draped with the flags of all nations. The eagle of Prussia floated in the midst of the colors of France, and the Turkish crescent fraternised with the banner of England. M. Bouillaud presided, supported on the right by M. Gavarret, on the left by M. Tardieu, and pronounced an eloquent address, whose feeling was responded to by every member of the great assembly. When the illustrious professor said, "I cannot contemplate this scene without being profoundly moved; I feel my feeble powers fail to express the just sentiment of the occasion," all the audience replied by bravos the most sympathetic; and when the orator concluded, "Let us rise to salute these entwined flags, and then unite our hands as they are united, in sign of complete and cordial fraternization," the enthusiasm was at its height, and the amphitheatre resounded with a thunder of applause.

But alas! having been at its height, it was all the more liable to fall. The day was very hot, and the old amphitheatre was constructed for other purposes than those of ventilation. The question of the day was tuberculization; and after the reading of the first memoir, the audience began to reflect, to calculate that many more were to follow, that they were "in" for three or four hours at least. People grew restless and anxious. Presently

every one was electrified with a voice, whose timbre, entirely exotic, pierced right through the decorum, of the assembly.

"M. President, is it permitted to ask a question?"

"Certainly; speak."

"I am a stranger; I am a physician from Holland, and as a Hollander I have been invited to assist at the International Congress, but I find I have made some mistake; for in my opinion this is no congress, but a class, a school-room, where some doctors have come together to admire each other, and hold themselves up for admiration."

Literally, that is what the honest Dutchman said. He spoke with all the traditional phlegm of his race; he scanned each word, and the ironical syllables fell into the midst of the "band of brothers" like so many bomb-shells. Of course there was confusion, and calls to order; then, finally, the reading of the papers on tuberculization was resumed.

The other questions that will occupy the Congress are as follows:

Second Session.—Continuation of the discussion on tuberculosis. Discussion on the influence of climates, races, and different social conditions upon menstruation in diverse countries.

Third Session.—On the constitutional accidents which occasion death after surgical operations.

Fourth Session.—Is it possible to propose to different governments efficacious measures to restrain the propagation of venereal diseases?

Fifth Session.—On the acclimatation of the races of Europe in warm countries.

Sixth Session.—On the influence of alimentation upon the production of certain diseases in different countries.

(The memoirs announced upon this question all relate to pellagra.)

And in the same séance will be developed some considerations upon entozoa.

This programme promises well, some of the topics being of extreme interest and importance, and only capable of being studied in the light of the experience of physicians of many nationalities. All the medical world that is not at the congress has gone into the country, whither we will follow them, and collect some gleanings from the rich harvest of the provincial socie-

ties, which in intelligence and learning are not inferior to those of Paris.

The Contagion of Cholera.

The Imperial Society of Medicine at Lyons, in the séance of the 15th of July, listened to a dissertation by M. Rodet, upon the capital question of the contagion of the cholera. M. Rodet, who occupies a middle ground between the non-contagionists and the contagionists, commenced by citing a certain number of facts that had been adduced by each party in proof of its theory. On the side of the first, four. In 1835, the vessel Ville de Marseille was stationed two or three miles from Toulon, where the cholera was then raging, and the crew had frequent intercourse with the infected city, yet not a person took the disease. In 1831, among a hundred nurses and attendants upon cholera patients in the hospital at Cairo, not a single person took the cholera; eighty nurses in the hospital of Monsourah have enjoyed the same immunity; and among sixty at the hospital at Damiette, only one took the cholera. Again, at the Hospital of the Dey at Algiers, the immunity of the persons attached to the cholera wards was so great in 1865, that one might have supposed them to be asylums of refuge. Finally, in the Military Hospital of Constantinople, 1,488 cholera patients were received from the 27th of January, 1855, to the 31st January, 1856, of whom 658 died. Their clothes and linen were washed by the hospital attendants; the privies exhaled from time to time fetid emanations, which spread throughout the hospital, and even beyond its precincts; yet in spite of so many conditions favorable to contagion, the disease was not communicated to any other patient, or to any of the persons attached to the wards.

It is noticeable, however, in connection with the first case cited, at Toulon, that although the crew of the Ville de Marseille was so remarkably spared, twelve physicians succumbed to the epidemic. In 1865, there perished in the same city six physicians, two apothecaries, ten nurses at the marine, and five at the military hospital, in all twenty-three persons connected with the care of the cholera patients.

In these cases, however, the non-contagionists may still urge, that the victims were at the same time exposed to epidemic influence, so that it is impossible to tell what share contagion

had in the infliction of the disease. This argument does not hold in regard to seven other cases quoted by M. Rodet, occurring during various epidemics, and one related in detail by M. Petiteau, that he observed last September. In all these cases the infection seemed to be directly transmitted by persons going from an infected to a healthy locality, was first communicated to persons with whom they came directly into contact, and thence from individual to individual, over a certain radius, after which the morbid influence seemed to be extinguished. In only one case was a wide-spread epidemic excited. In five of these eight cases the disease was imported by people who had visited the infected locality, merely during a few hours or days, and were attacked shortly after their return home, communicating the disease to those who nursed them. In M. Petiteau's case the attendants on the patient escaped, but after his death a drunken comrade, who persisted in passing all night by his corpse, embracing it, and committing a thousand extravagances, was speedily smitten. Twelve cases followed this infection, of which six died. In the cases cited by Rodet, for the first, only the son and husband of the original patient died, while she recovered, and the disease went no further. In the second instance, only the mother of the patient was carried off, while he recovered. In the third, fourteen persons perished out of a population of 130 inhabitants. In the fourth, there were thirty-one deaths in thirty-four days. The other three instances of infection mentioned by Rodet were occasioned by the flight of persons from places where they had lived, for some time during the prevalence of the epidemic, into healthy localities. In the first case, a general epidemic was lighted up. In the second, all the members of a family living in different houses were successively attacked. In the third, twenty-seven persons were attacked, of whom twelve succumbed.

M. Rodet, although attaching full importance to these facts, as proof of the communicability of cholera by direct contagion, is careful to point out that such influence cannot explain all the bizarre phenomena of epidemics, and that it is necessary to admit, over and above the focus of infection, a general cause which hovers over all the individuals placed in this focus, an epidemic cause, a *quid divinum* or *ignotum* as has been so often repeated.

The *Medical Gazette* of Algiers reviews a recent work by

M. Jules Girette, where this question of the epidemic influence is treated on the largest scale. This writer, by the very title of his work, Civilization and the Cholera, betrays that his views are liable to be all rather biassed by the idea that belief in contagion must tend to barbarize nations, and hence ought to be discountenanced on moral grounds. It is rather unfortunate that this initial bias should be so perceptible, for it somewhat tends to shake the reader's confidence in the complete impartiality of the author's statements. Yet various circumstances pointed out, concerning the march of the epidemic of 1865, along the shores of the Mediterranean, seem certainly difficult to reconcile with the theory of the perfect efficacy of quarantine. "Greece and Sicily isolated themselves completely, and escaped the cholera. But so also did Corsica, which continued to communicate freely with the infected cities of Nice and Livourne, and only subjected vessels coming from Marseilles, where the epidemic was at its height, to a quarantine of three days. Salonica and Volo, unexpectedly exposed to the contagion, after a prolonged quarantine, nevertheless escaped. Neither Sarnsoum, nor Catourn, nor Dourgas, nor Varma were attacked by the cholera, although they were constantly visited by emigrant vessels. It scarcely touched Trebisond, traversed by hosts of fugitives *en route* for Persia. Yet all these ports had no other defence than a quarantine of from three to five days. At Malta, Beyrout, Dardanelles, and Odessa, the epidemic was communicated to the city by the lazaretto that professed to protect it. At Constantinople, a Turkish frigate evaded the quarantine, and imported the disease. Majorca, surrounded by a *cordon sanitaire*, attributed the cholera by which it was decimated, to some secret fraudulent importation, since no other cause could be discovered. The same with Alicant. At Enos the epidemic raged, and could be explained by no suspected communication. Trieste, spared up to the 28th of September, and believing itself secure behind a model lazaretto, awoke to find the cholera within its walls. Southampton, freely open to arrivals from Alexandria, did not register its first death from cholera until the 25th of September, nearly at the same time as Trieste, and two months after Marseilles."

M. Girette, however, takes great pains to trace the march of the epidemic of 1865, from its cradle, among the hordes of pil-

grims to Egypt. M. Jobert, however, sanitary physician on board the Arethusa, who reviews the book, lays much more stress than the author upon the fact that some new and peculiar atmospheric conditions, or epidemic capacity, must have prevailed at Egypt during that year, since every year the pilgrims were in the habit of having the cholera at Hedjaz, but it was not communicated beyond their own camp. M. Jobert quotes with especial emphasis the description given from personal observation by M. Girette, of the state of things at the temple of Withoba, at Punderpoor, where men and women were crowded together by thousands, in a narrow court, awaiting their turn to enter the temple. Inside the little stone temple the same, and worse; the emanations from the bodies of the worshippers condensed upon the statue of the god, and the moisture was regarded as a miraculous sweat! The resident physician at Punderpoor believes that the first origin of the cholera is probably at this celebrated shrine.

To return for a moment to M. Rodet. He speaks hopefully of the good effects of the treatment suggested by Dr. Burg, and in 1865 experimented by M. Lisle, physician at the Insane Asylum at Marseilles. Upon the appearance of the epidemic in the asylum, M. Lisle had at first endeavored to combat its ravages by the ordinary method of diffusible stimulants. He lost twelve patients out of fourteen, a number much greater than the ordinary average, and whose excess is to be attributed to the much feebler resistance to the disease offered by the insane. Finally the servant of M. Lisle was attacked; he employed the same treatment, and with equal lack of success, for at the end of twenty-four hours all hope seemed to be lost. In this extremity he resolved to try Dr. Burg's prescription, and considerably to his surprise the woman recovered. He then applied the same treatment to the remaining patients in the wards, and the results surpassed his expectations. Among twenty-six men he obtained twenty-one recoveries, and among six women (including his servant), four; in all, twenty-five cures among thirty-two cases.

The following is the formula for the remedy that obtained such unlooked-for success:

Dissolve five per cent. of sulphate of copper in 150 grammes (about five ounces) of distilled water; and add to this 150

grammes of sugared water, together with 10 drops of Sydenham's laudanum.

A Case of Osteomalacia.

At the Society of Medical Sciences at Lyons, was recently presented by M. Vérard, a most interesting case of osteomalacia. The patient, as usual, a woman, was thirty years old, and had been the victim of the disease for ten years at the time of her death, which occurred in an attempt at child-birth. I have been unable to find the details of the case as related by M. Vérard, having only at hand a subsequent report upon the case, made by Dr. Berne, surgeon at La Charité at Lyons.

In this report is only noticed, that the commencement of the disease had been characterized by sharp pains, which had been supposed to be rheumatismal; that the pregnancy had, as usual, greatly accelerated the march of the disease; that the diseased bones presented were all highly porous; that in the spongy tissue, the osseous trabeculæ had become rare, or had disappeared; the medullary spaces had united together, and in the hollow bones contributed to enlarge the medullary canal; that even in the cortical compact substance, the vascular canals were enlarged, and formed areolæ, which uniting transformed it into a spongy tissue of large network; which indeed was so general that the compact tissue had almost disappeared, and there only remained the superficial layer, which, moreover, was infiltered by a yellow, fatty, medullary substance; that, besides, in the parts of the osseous system which were the most altered, were discovered numerous cells resembling pus globules.

This last fact seems to confirm the opinion of Virchow, who ascribes osteomalacia to a parenchymatous inflammation, the immediate consequences of which are only an interstitial exudation, but the remote result is the destruction of the osseous tissue.

A chemical analysis of the bones was made, principally with a view of searching for lactic acid, and thus indirectly testing the theory[1] that ascribes the resorption of the lime salts to the presence of this agent. It was impossible to find lactic acid in the free state, for at the time the analysis was made, the bones had already submitted to maceration for several days in water

[1] Of MM. Marchand, O. Schmidt, and Otto Weber.

saturated with marine salt, and the acid, if present, would necessarily be dissolved. But some lactates might still be left. To settle the question, the ashes of the calcined bones were treated with water, thus losing a considerable portion of their weight (0.42 gr. out of 0.99 gr. for the spongy substance, and 0.23 gr. out of 1.73 gr. for the compact). The filtrated substances, precipitated with nitrate of silver, gave 0.20 chloride of sodium in the first case, and 0.10 in the second. Remained 0.22 and 0.13 of residue, in which, if anywhere, the lactates were contained. In this residue, dissolved in distilled water, the presence of an organic acid was presently proved by the addition of a few drops of nitric acid, then lime-water, which formed a precipitate, proving that the nitric acid had found material to convert into oxalic acid, which produced an oxalate with the lime. Further examination showed that the solution did not precipitate with baryta-water, had no action upon lime-water until it had been treated with nitric acid, and gave a white precipitate with concentrated acetate of zinc, whence the presence of lactic acid was conclusively proved.

The usual disproportion between the organic and inorganic materials of the bones was also shown by the analysis. The proportion in 100 of the inorganic matter instead of being 64, the normal figure for compact bone, was 41, and in the spongy substance not more than 18.

The proportion between the carbonates and phosphates remained the same, the former being one-tenth the weight of the latter.

M. Vérard very justly regretted that no experiments had been made to ascertain whether, in spite of the narrowness of the basin, the head of a fœtus could not have been made to pass, in virtue of the softness of the bones. The antero-posterior diameter of the inferior strait only measured from a centimètre and a half to two centimètres; but the bones were so soft, that the first placed in the pelvic cavity easily forced a place for itself. Dr. Berne thought that in a similar case, at a moment of *accouchement*, before the obstetrician should address himself to the cesarean operation, he should seriously consider whether the pelvic basin were not susceptible of enlargement by dint of pressure. In the case in question, I am unfortunately unable to tell what was actually done.

The Function of the Vascular Glands.

At a recent séance of the Academy of Medicine in Belgium, Dr. Foisson read a paper, propounding a theory on the function of the vascular glands, that seems to me much the most ingenious and complete of any that has ever been advanced concerning them. This theory carries out the suggestion made by Broussais, who assigned to the spleen the function of deviating the blood from the stomach; so the thymus and thyroid, a similar rôle for the respiratory organs. This idea, however, being based upon no serious proof, passed unperceived. But M. Foisson has greatly enlarged and strengthened it in his essay, of which I shall endeavor to give you an idea.

The general theory of derivation is the following: All organs submitted to alternations of action and repose, require a greater amount of blood during the first than the second period. The variations thus necessitated in their circulation, are effected by an agency independent of the general circulation, namely, the appropriate vascular glands, that act by driving the blood away from the organs when they have no need of it.

The only organs in adult life, engaged intermittently in active functions, are the muscles, stomach, brain, and uterus.

The muscles, when acting separately, mutually derive the blood from one another, and when they act all together, the heart quickens its action, and sends the excess of blood required. Their variations, therefore, depend directly on the general circulation, and they have no need of special apparatus.

But the stomach is essentially intermittent in its activity. The secretion of gastric juice evidently demands a large amount of blood, to judge from the size of the arteries distributed to its walls. During the intervals of digestion, these arteries are tortuous, and comparatively little blood passes through them. The blood from the cœliac axis being mainly distributed by the splenic artery to the spleen, the tortuousness of this splenic artery may be supposed to be unfolded at an opposite time from that in which the gastric arteries grow straight.

The thyroid gland is the derivative reservoir for the blood going to the brain. This blood arrives at the thyroid from the superior thyroidien given off from the internal carotid—and the inferior thyroidien, that springs from the subclavian close by the origin of the vertebral, so that by a double route the circulation

of the thyroid can affect that of the encephalon. In virtue of that same connection, between the thyroid and the brain, do persons affected with goitre so often become cretins; the exaggerated development of the thyroid interferes with the nutrition to the brain, and the more important organ is actually starved out by the fraud of the less, which seizes its supplies *en route*. Finally, for the uterus, the mammary glands perform the office of derivation, and, after parturition, when the uterus must retract, and has no further need of the expensive nourishment upon which it has subsisted during pregnancy, the epigastric arteries, prepared for the task by the development they have experienced during the last months of this period, intercept the supply of blood going to the uterus, and convey it to the glands, by means of their anastomoses with the mammary arteries. Among animals in whom the mammary glands are abdominal, the epigastric artery supplies them directly. To this extremely suggestive interpretation of the well-known facts of the case, one difficulty may be addressed. If the extra nutrition of the uterus and mammary glands is carried on at alternate periods, how does it happen that the glands increase during pregnancy? This fact, however, is really provided for by the theory which admits that the satellite organs do increase coincidently with their principals, if only for the sake of being at hand, and in good condition, to receive the brunt of their circulation when the functions of the principal organ is intermittent; but that in addition to this parallelism of development, comes the alternative, or contrast, at the moment when the principal organ subsides into inactivity, and the satellite starts into full activity.

For explanation of the office of the thymus gland and suprarenal capsules, the theory is identical, but applied as it were in an inverted fashion as respects chronological order. The lungs and kidneys do not function at all[1] during fœtal life, and hence have need of only so much nutritive fluid as is required for their growth. But as they begin to act at the very moment of birth, the new supplies necessary for the maintenance of their functions must be stored up close at hand, ready to be turned into their future channels. For this purpose the thymus gland and suprarenal capsules are contrived. The blood during fœtal life is

[1] This is the remark of the author. But I believe it is not strictly correct for the kidneys, since the bladder is found to contain urine before birth.

directed towards them, as it were next door, but at the moment of birth the current is turned into the neighboring arteries, and from that moment the fœtal organs begin to waste and gradually disappear.

The thyroid gland also, though in action throughout life, is much required during infancy, since the brain at that period, as far as regards its intellectual functions, is in a quiescent or at least passive state, consequently the thyroid gland of children is proportionately much larger than in adults.

M. Foisson refers to the characteristics common to the structure of all the vascular glands, as tending to confirm his theory. Huschke and Kolliker agree in recognizing in all these organs the existence of:

1. A foundation system of trabeculæ, serving for a support to the vessels.

2. Vesicular cavities occupying the interstices left between the trabeculæ.

3. The presence in the cavities of a liquid charged with globules, and the absence of any efferent canal. Nothing in this structure suggests the idea of a secretion appropriated to the perfectionment of the blood or lymph, while it is on the contrary marvellously adapted for the purposes of derivation.

The entire theory is resumed in the following propositions:

1. All the organs of the economy consume during the periods of their activity an amount of blood more considerable than that required in repose.

2. In the normal state, the heart sends at each moment the same quantity of blood in every branch of the arterial tree.

3. The blood which arrives at organs in excess during their period of repose, is received by special organs called *derivators*.

4. The function of derivation may be performed without the intervention of an organ exclusively devoted to the task, as in the case of the mammary glands.

5. Every organ whose function is intermittent, possesses an apparatus for derivation.

6. Derivation is not only arterial, but sometimes venous, as when the spleen receives the *trop plein* from the portal vein, or the thyroid, during muscular exertion, from the engorged jugulars.

7. Derivation is a complementary function of the circulation, and necessary to a regular distribution of the materials of

nutrition and secretion. At the same time it is not absolutely essential to life, so that in the lower animals any of the vascular glands may be extirpated with impunity.

8. Derivation is explained by the following law of physics: When a pipe traversed by a fluid is divided into two branches, that of the two in which the current is the most rapid receives a greater quantity of liquid than the other.

This theory is so perfectly captivating to me, that as yet I have not been able to imagine any serious objection to its soundness. Perhaps you or your readers may be more critical, and I submit it to your judgment.

Union by First Intention after Lithotomy.

Professor Bouisson, of Montpellier, is at present writing a series of articles in the *Montpellier Médicale*, upon union of the wound by first intention after the operation of lithotomy. The Professor not merely believes this to be possible, and in his first paper adduces four cases in proof of his assertion, but engages to show how this very desirable result can be secured. Of these four cases, the first was that of a young man who had been treated for some time with elastic bougies, in the hope of sufficiently dilating the urethra to admit of the operation of lithotrity. All at once, however, the patient became unquiet and irritable; an obstinate spasmodic condition of the canal joined itself to the organic retraction, and forced the surgeon to abandon all hope of crushing the calculus, and an operation for lithotomy was decided upon. Owing to the presumed smallness of the stone, the median incision was selected. The operation was performed on the 16th of December, and encountered no serious difficulties. An incision of three centimètres practised on the median line of the perineum easily attained the urethra. After division of the cutaneous and cellular layers, the membranous portion being directly divided, the length of the left edge of the catheter which had been introduced in the urinary canal to serve as a guide, a lithotome was introduced, the catheter withdrawn, the finger, gorget, and forceps successively introduced into the bladder, and the calculus seized and extracted.

The calculus was spheroidal, with irregular surface, so compact and hard that the operation of lithotrity would have been very difficult. A vesical injection terminated the operation,

which had been performed with the assistance of chloroform. The knees of the patient were then drawn together, and maintained in an elevated position by a cushion placed underneath; a calming and diffusible draught was administered; the day passed without fever or vesical pain; the patient vomited twice; in the evening a little urine escaped by the natural passage. The next day reddish urine was passed naturally, also a very small amount escaped at the wound, whose appearance was good. During the two following days also the local and general phenomena were satisfactory; the urine nearly entirely passed by the urethra. The wound closed without suppuration, and by the eighth day was *completely cicatrized by first intention.* The cure was permanent.

In the second operation performed by M. Bouisson, the patient was sixty-four years old, and the bladder contained six calculi and was completely paralyzed. After the operation, whose details I will not repeat, the persistent retention of urine, which did not even escape by the wound, rendered it necessary to leave a sound permanently in the bladder, for the accumulation caused much pain and suffering to the patient. By this means also the urine was completely turned away from the wound, a circumstance which undoubtedly favored its union, which was effected in six days, by first intention, without any trace of inflammation or infiltration. In this case also the median incision had been practised. The third, the same form of the operation. The subject was sixteen years old; the calculus, though hard and voluminous, was ovoid, and presented itself to the forceps by its most favorable diameter, so that it was extracted without difficulty. After the operation, the adduction of the thighs was secured by means of an apparatus, so that the lips of the wound were brought in contact, and the dorsal decubitus strictly enjoined. The first day only the urine escaped by the wound; after that the patient was able to urinate voluntarily. Even after the subsidence of the swelling around the lips of the wound, which might at first have opposed the escape of the urine, that liquid continued to traverse the natural passages, and owing to this fortunate circumstance the wound was cicatrized by the sixth day. Neither infiltration, nor ecchymosis, nor suppuration supervened, and the cicatrix remained perfectly solid. The fourth operation, with the medio-lateral incision, was performed

on a child of six years old, who had suffered from painful micturition from the age of two years. In this case the first sounding had failed to discover the calculus, and although that was distinctly perceived at the second examination, it seemed again to disappear at the moment of the operation. Nevertheless, M. Bouisson made the incision. The posterior radius of the prostate gland seemed so short, in consequence of the flattening of this organ, that, having practised the median section of the skin as far as the urethra, M. Bouisson judged it prudent to incline the lithotome in the direction of the oblique radius of the prostate, in order to avoid the rectum, and to limit this oblique section to the gland, so that the incision represented a broken line whose first part was straight, and the second oblique. This opening, more than sufficient for the extraction of the calculus, gave issue to a certain quantity of urine, which carried the stone along with it into the very grasp of the forceps. The calculus was the volume and shape of an olive; mammillated, reddish-yellow, and composed of uric acid. The whole operation only occupied three minutes from the moment of the incision to the extraction of the stone.

In consequence of the inclination of the lithotome, a branch of the perineal artery had been divided, giving rise to considerable hæmorrhage, an accident that had been entirely avoided in the other operations. The hæmorrhage was arrested by torsion of the vessel, but returned some hours after the operation, to be finally vanquished by compression and the application of ice. This was the only notable effect of the operation. The urine escaped by the wound during the evening and in the night; but after the first day the passage of urine ceased to be continual, and came under the influence of vesical contraction. Towards the end of this day a part of the urine passed by the urethra, and from the fourth day no more escaped from the wound, which united without suppuration, and without the occurrence of either sanguine or urinary infiltration. By the eighth day the cicatrization was complete.

All these cures were obtained by the perineal operation. M. Bouisson thinks that such happy results could rarely be achieved when the hypogastric incision was practised. In succeeding papers he hopes to develop further views suggested by the interesting observations of which I have related the summary.

New Apparatus for Irrigation of the Eye.

Dr. Amable Cade, of Saint Andéal, also makes a communication to the *Montpellier Médicale*, concerning a new apparatus devised by himself for securing continual irrigation of the eye after the operation for cataract. This is composed:

1. Of a hemispherical reservoir, of a capacity of nearly a quart, with an opening at the top, and capable of being suspended over the head of the patient.

2. Two supra-ocular recipients, of lozenge shape, each furnished with two little handles, destined either to fix the apparatus before the eyes by the aid of a circular band, or to keep in place the two recipients when both eyes have been operated the same day. Their posterior side is made of gold-beater's skin, which ought to be placed in immediate contact with the closed eyelids.

3. Two tubes, communicating between the reservoir and the recipients. These tubes are furnished with screw joints, which permit the suppression of one of the recipients when only one eye is operated.

4. Little pieces of sponge loosely introduced in the communicating tubes, to prevent the passage of the water, except drop by drop, every second in ordinary cases. These sponges may be removed in case of imminent danger from violent inflammation, when a rapid current of cold water is needed.

5. Two discharging tubes, a yard and a half in length, destined to conduct the irrigating fluid from the recipients to a vase placed by the bedside.

By means of this apparatus, Dr. Cade has already performed eight operations for cataract with the most complete success, in some cases warding off a commencing phlegmonous inflammation, that threatened to become a terrible complication.

P. C. M.

The International Medical Congress.

PARIS, Sept. 9.

To the Editor of the Medical Record.

SIR—Now that the International Medical Congress has come to an end, it may not be inappropriate to review its proceedings, and endeavor to form an estimate of its results.

This task cannot fail to disappoint. It is acknowledged on

all hands that the Congress was ill-organized, the programme arranged without sufficient tact, and the legitimate aims of the discussions almost entirely lost sight of. Evidently the great advantage to be gained by the discussions of an assembly of physicians from all parts of the world would be, that the contingent of information furnished by each should represent something peculiar to his country or school. Data, often painfully gleaned from the records of travellers, would be collected in abundance by medical observers resident on the spot, and offered to enrich the common treasure. Moreover, celebrated men, who had hitherto talked to each other across seas, and through the medium of books, would meet face to face, would familiarly converse with each other on the mighty labors by which their names, their fatherland, had been rendered illustrious, and derive mutual refreshment from the rare intercourse.

All this I say might have been expected. But the expectation has been very imperfectly fulfilled. In the first place only inadequate provision was made in different countries to send such men as should most justly represent the actual condition of national science. There should have been official delegates from the principal universities, who should have been distinguished from the crowd of mediocrities who might choose to attend, but who should not be mistaken for such representatives. From lack of such precaution, a multitude of opinions were advanced which were entirely undeserving the sanction of so solemn an occasion as this professed to be. Any one could speak, and any one did speak; and, as a rule, the more distinguished visitors held their tongues.

Not a word from Virchow or Graefe, who were both present; not a word from Bennett or Simpson. Indeed only two Englishmen are on record as having spoken, and not a single American. The debates were chiefly maintained by the French and Italians. This was probably in part owing to the very imperfect knowledge of French that prevails among us Anglo-Saxons, especially the Americans, and which, as I have had quite frequent occasion to observe, seriously interferes with the benefit they are able to derive from a few months' visit to Paris. But the silence also resulted, in all probability, from the fact that few had prepared themselves for a sufficiently long time in advance; as a consequence, the topics for discussion were developed in the most

unequal and irregular manner. The minute anatomy of tuberculosis occupied two or three sessions, in which nearly all the speakers were French, who revived old disputes without reporting any researches made especially for the Congress. On the other hand, the three questions that seemed most peculiarly adapted for international discussion—the influence of various climates upon menstruation; the problem of acclimation; and the influence of alimentation—were only touched upon in the most cursory manner. As a whole, therefore, the Congress cannot be said to have arrived at any valuable result. Nevertheless, two of the discussions—on the treatment necessary to prevent purulent infection after surgical operations, and on measures to be recommended to the governments of various countries to arrest the spread of syphilitic diseases—were exceedingly interesting; and in all the *séances*, various topics were incidentally developed that are quite worth recording. I shall endeavor to mention some of the principal, beginning with those which occupied the least time and attention.

The communications made on the subject of alimentation were, in accordance with the programme, all written in reference to pellagra. M. Bouchut has found on grains of wheat spoiled by the damp, a fungus very similar to that found in the same circumstances on the maize, to the consumption of which pellagra is generally attributed. He proposes to name this fungus *sporisorium tritici*. To obtain it, it is only necessary to place some wheat in a jar, and keep it damp.

M. Demaria believes that pellagra is not dependent on an accidental poisoning, but is a constitutional neurosis, dependent on hereditary influences and poor food.

The communications of M. Dropsy, of Cracovia, concerning the Polish Jews, and Mr. Kingston on the Anglo-Canadians, presented in the course of the discussion on tuberculosis, touch on a subject of more widely spread interest than the poison of the maize. Each tends to prove the enormous influence of animal food upon the preservation of health, especially from the ravages of phthisis. At Cracovia, the peasants are all healthy and robust, living much upon animal food. The Jews scarcely spend more than two sous a day for their nourishment, and never eat meat. Consumption makes such ravages among them that the race threatens to die out. In the same way in Canada, the

French Canadians, who eat meat in excess, often three or four times a day, are declared by Mr. Kingston (an Englishman) to be a superb race of people; while their English neighbors, who live much more soberly, are infinitely more subject to tuberculous disease.

The question of acclimation was as much restricted as that of the influence of food, being limited to the investigation of the conditions necessary for acclimating Europeans in warm countries. M. Simonot read an interesting memoir on the subject. For him the difficulty did not arise from the heat of the climate, but the poisonous influence of miasm. Wherever that could not be destroyed, it was useless to expect to make permanent homes for white families.

M. Lombard, not adhering strictly to the question, communicated the result of researches on the laws of mortality in Europe, according to atmospheric influences. According to these, winter and spring is the most sickly season for all the north and centre of Europe, while the southern countries enjoy their excess of mortality in summer and autumn. In Europe, miasm still continues to be one of the most powerful agents influencing mortality, and it is an agent which in this country it is in the power of man to remove.

A number of carefully prepared memoirs on the question of menstruation were communicated; but, as most of them consisted mainly of statistical tables, they could not be read. The statistics that *were* read, by M. Lagneau and M. Joulin, accord very well with the established law, in virtue of which menstruation is known to be precocious in warm climates, and retarded in cold. In English India, the average age for the establishment of puberty is twelve years and six months. In Norway, sixteen years and four months. The supposed differences between different cities of France is shown to be trifling, Marseilles being only six months earlier than Paris.

Mr. Robert Cowie has made some curious researches upon menstruation in the Shetland Islands, and its connection with longevity. In this locality the menses are established at the same age as in Great Britain, while the *ménopause*, instead of occurring at forty-five or forty-six years, is deferred to a period varying from forty-eight to fifty-four years, fifty-one being the average. In connection with this, Mr. Cowie notices a consid-

erable difference in the rate of mortality, as shown by the following table:

	Shetland Islands	*Scotland*
Above 70 years	= 33.55 per 100.	18.25 per 100.
" 80 "	= 20.00 "	7.05 "
" 90 "	= 5.03 "	1.00 "
From 95 to 105 years	= 2.68 per 100.	0.29 "

The discussion on tuberculosis was divided into three parts, severally referring to its pathological anatomy, its prevalence in different climates, and its treatment. Of these, the first received much the most attention, not because of its superior importance, but because it happens to be extremely *à la mode* at the moment, and more speakers had something to say on it. The debates touched on the following questions: First, the specificity of the tubercle; second, its identity with the products of inflammation; third, the precise seat of the granular deposit; fourth, the relation of the yellow degeneration to the gray or crude tubercle; fifth, and finally, two or three peculiar and rather bizarre opinions were advanced which had no relation with any of these points.

The question of the specificity of the tubercular deposit may be variously regarded. A special anatomical element may be sought, as characteristic of tubercle, but such an attempt was universally pronounced to be chimerical. On the specific character of the tuberculous product, either the gray or the cheesy may be attacked or defended, together or separately. Such a combat occurred, and was marked by a diversity of arguments, in support of a diversity of theories. Professor Crocq of Brussels, and M. Lebert, assimilate completely the tuberculous process to the inflammatory. M. Crocq began by declaring that the cellules of the gray granulation could be compared to nothing but the cellules of the lymph and lymphatic glands, the white globules of the blood, of mucus, and of pus; in other words, leucocytes, among which he did not hesitate to class them. In the granulations, these leucocytes are distinguished from pus, chiefly by the absence of intercellular substance; are small, because bathed by no liquid, and have only a single nucleus, on account of their low vitality. These leucocytes arise from the epithelial cells, or those of the connective tissue, and submit ultimately to fatty degeneration, etc.

The phenomena successively exhibited in the formation of these leucocytes, are identical with those of the cellular elements of inflamed tissues. When an organ is examined in which tubercles are developing, it is found strewn with vascular patches. Sometimes the centre is already consistent and elastic, and at this centre the tubercle is gradually formed by exudation, since vascularization and repletion of the tissues by matters destined to be exuded, is common to inflammation and tubercular formation. Moreover, in inflammation the cellular elements absorb new material, swell, become opaque, and finally give birth to new generations of cells similar to the leucocytes. These, either in the tubercle or inflammation, have four destinations. First, they are destroyed, and their materials reabsorbed; second, they are transformed into new connective cells; third, they swim in an intercellular liquid, and constitute pus; fourth, they undergo the fatty degeneration.

It results from these considerations (concludes M. Crocq), that tuberculization is by no means a specific disease, recognizing a vice of the blood for cause, but an affection of the same order as inflammations, and should be combated, like other phlegmasias, by antiphlogistics and revulsives.

Lebert's views are substantially the same, but are based on inferences derived from certain experiments made upon animals by injections of various substances under the skin. In eleven instances were used the products of chronic pneumonia, chronic adenitis apparently tuberculous, and tuberculous granulations of the lungs; two experiments with injections of pus; nine, the products of expectoration and of pulmonary caverns; ten injections of charcoal or mercury were made into the jugular vein.

The charcoal produced little *emboli*, followed by cellular hyperplasma, little granulations, and even multiplication of the epithelial cells and those of the connective tissue. The mercury provoked, besides, an inflammation of the vessels; here also, however, cellular hyperplasma, in the form of little granulations, and, when the irritation had reached a high degree, formation of solid inflammatory foci which ultimately suppurated and produced caverns.

The inoculation of morbid products excited a more severe local irritation, and also numerous granulations in different organs.

Hence, for Lebert, the tubercle strictly resembling the granulation thus artificially created, is a product eminently hyperplastic, and cannot be classed with accidental products properly so called.

After this exposition of the pure inflammatory doctrine of tuberculization, Hérard and Cornil rushed to the defence of their theory, which may be called modified inflammatory. For them, the gray granulation is the only characteristic lesion of tuberculosis which excites an inflammation, whose degeneration constitutes the so-called cheesy tubercle. Neither of these champions undertook the task of rebutting the views of Crocq or Lebert, but each addressed himself to that side of the doctrine which touched upon, and was contradicted by, that of M. Villemain. This physician has recently made some remarkable experiments on the inoculation of tubercle, and has succeeded in thus conveying the disease to rabbits. So far, his experiments tended to confirm (at least without the criticism afforded by those of Lebert) the doctrine of the specificity of the tubercular deposit. But, proceeding further, he professes to have obtained gray granulations, after inoculation with the yellow cheesy matter. In consequence of this, he renounced the views he had previously held in regard to that substance, and, no longer believing it to be a secondary inflammation, he concluded it to be a more advanced stage of the crude tubercle, thus returning frankly to the ideas of Laennec. Hérard replied that this cheesy pneumonia (*pneumonie caséeuse*) might be sufficiently stamped with the character of the granulation by which it was caused, to serve as material for infection; but such did not prove that it was identical with the granulation which could often be found in its midst, little changed.

M. Cornil attacked Villemain on another point, namely, in regard to the *seat* of the granulation. Villemain, in a memoir of some length, read at the first session of the Congress, declared that the greater number of granulations occupy the air-vesicles, herein again coinciding with Laennec. At the beginning of his researches, he had considered the contents of the alveoli as a product belonging to the pulmonary epithelium, and distinct from the granulation, which is the view actually held by Hérard and Cornil. But subsequently, M. Villemain became convinced that the membrane of separation between the alveoli was not

homogeneous, but contained a special element identical in structure with the connective tissue. In this tissue were deposited the greater number of the granulations. He considers the existence of an epithelial layer at the internal surface of the alveoli to be extremely problematical.

Hence, he does not believe that the elements constituting the catarrhal or cheesy pneumonia are derived from epithelial cells, but from the nucleated cells of the membrane separating the alveoli. Being much crowded, these cells sometimes assume plane surfaces from pressure, so as to resemble epithelium; but they are never soldered together.

M. Villemain admits that the initial stages of tubercle resemble those of inflammation, inasmuch as the two external zones of the three that constitute a tuberculous nodosity, represent cells in different stages of development; but the two processes are to be distinguished by the terminations, which for inflammation is pus, for tubercle fatty degeneration. The similarity between the anatomical elements of these two states is, as M. Villemain justly thinks, no reason for identifying them.

M. Cornil denied point-blank that the tubercle was developed anywhere but in the lymphatic or adventitious tunic of the blood-vessels, especially at their bifurcation. This phenomenon (in tuberculization of the pia mater) is accompanied by two others: 1st. The multiplication of similar elements in the connective tissue of the pia mater which surrounds the diseased vessel; 2d. The coagulation of the blood, and the retrograde metamorphosis of the fibrine and blood-globules.

M. Cornil admits that in the lungs there is a development of elements in the interalveolar membrane. But, besides, he insists that the large pavement cells, perfectly free, measuring 0.015, are really epithelial, and cannot be confounded with the elements of the connective tissue, which are small, 0.004, agglutinated, intimately united by a homogeneous and granular substance. The first constitute the tuberculous pneumonia; the second the granulation.

A Hungarian physician, Dr. Bakody, warmly supported the views of Cornil. He moreover suggested that the tubercle developed especially in the summit of the lungs, because there the respiratory movements are less extensive, and the lungs can-

not readily reject the mass of cells which form in the alveoli in consequence of inflammatory irritation.

The question concerning tuberculization in different countries and circumstances was then taken up. M. Marmisse read a memoir upon the influence of this disease on the mortality at Bordeaux. The influence of hygienic conditions is indicated by terribly eloquent figures. Among 1,000 poor people registered at the *Bureau de Bienfaisance*, 625 die of phthisis, while the rich classes only yielded a tribute of 87 on 1,000 to this formidable disease.

I have already quoted M. Dropsy's remarks on the Jews in Poland, and Mr. Kingston's on the English in Canada. Dr. Homan, of Christiania, read a memoir on the disease in Norway, and its distribution in different sections of the country. The proportion of deaths from tuberculous diseases in Norway is about 162 in 1,000. The variations in different districts are from 79 to 226 per 1,000. Sometimes a great difference is observed between two neighboring districts, which cannot, then, be referred to difference of climate. Dr. Homan invokes syphilis as a powerful agent to explain this difference. The capital question of the treatment of phthisis received no new light.

I must not forget to mention, among the opinions independently broached, that of M. Empis, who invents a new disease called *granulic*, distinct from tuberculosis; and of a physician whose name escapes me, who declares the cause of tubercle to be excessive pressure in the blood-vessels, whereby the colloid matters in the blood are exuded in the form of granulations.

The second great question, on the prevention of accidents after surgical operations, was developed with much animation. Two principal opinions obtained: one the perfect efficacy of local treatment, the other the importance of minute constitutional care.

One of the most interesting memoirs read in support of the first theory was that of Professor Bourgade, of Clermont-Ferrand, on the employment of perchloride of iron. The capital fact from which the Professor reasons is the different effect produced by wounds made with a bistoury or with caustic. The latter are habitually innocuous; the former often followed by serious accidents of infection. Some surgeons have sought on this account to substitute the caustic for the bistoury; but that is

impossible in a large number of cases, and the bistoury will always remain the surgical instrument *par excellence.* The problem is, therefore, to reduce the wound made by it to the same conditions as that produced by the caustic. This, according to M. Bourgade, is accomplished by means of the perchloride of iron, which combines intimately with the tissues, and forms over the wound a kind of magma solid and adherent, a species of plastic cuirass, which resembles both a coagulum and an eschar, which becomes hard and resistant, and only begins to separate by suppuration, the sixth, eighth, or tenth day after the operation. The following is the method for its application: When the operation is finished, and the arteries suitably tied, the wound should be washed and dried with the greatest care; and when the flow of blood is well arrested, the whole surface is covered with lint saturated in a solution of perchloride of iron at thirty degrees. It is essential that all parts of the wound, bones, muscles, cellular tissue, etc., receive the direct action of the liquid. The whole is covered with moistened lint.

When the tampons of lint fall, they show a blackish surface, covered with a thin eschar, which gradually detaches itself, revealing a pink wound in very good condition, already covered with fleshy granulations.

This method, of course, is only adapted to wounds uniting by second intention; but, in M. Bourgade's opinion, that is the only union possible in hospitals. Several surgeons expressed the opinion that the attempt to obtain union by first intention was rapidly being abandoned. The perchloride has been applied in 95 operations, all followed with success.

The accidents that are guarded against by the perchloride are more especially purulent and putrid infection, phlebitis, angeioleucitis, osteomyelitis, and consecutive hæmorrhages.

The perchloride is supposed to act by a light cauterization of the bleeding surfaces, and by effecting a solid coagulation even in the interior of the veins. There results an adhesive and obliterating phlebitis, which prevents the suppurative phlebitis, and opposes the absorption of morbid elements.

M. Barbosa, delegate from the Portugal government, read some extracts from an important statistical memoir on the operations practised for the last twelve years in the hospital St. Joseph, at Lisbon. They were quite favorable—only 59 deaths

among 243 amputations of limbs; among these, 62 amputations of the thigh, which gave 29 deaths.

M. Barbosa lays great stress upon the good hygienic conditions of the wards, ventilation, and cleanliness. He adopts the circular method for amputation, and always dresses the wound with lint dipped in alcohol saturated with camphor, an ancient custom in Portugal.

Professor Gosselin followed Barbosa in attaching much more importance to these circumstances of hygiene than to the local dressing. He takes especial pains with the *morale* of his patients, endeavoring gradually to accustom them to the idea of the operation, allowing them, whenever it be possible, to name the day, always securing them from pain by the use of chloroform, etc. He is also careful to remove the patient as far as possible from cases of erysipelas, etc., which, unfortunately at La Pitié, cannot always be very far. After the operation, he is especially careful to avoid doing anything to cause pain. Never places any apparatus on the stump which will render it necessary to lower or raise it; does not attempt to draw together the edges of the wound, and rejects the use of alcohol in the dressing to avoid pain; places the patients on a mechanical bed, which allows them to be moved without suffering. By these precautions, out of 48 amputations he succeeded in saving 29 patients, a mortality of 39 on 100. Of the 19 deaths, 10 only were by purulent infection.

As an instance of the disastrous influence of moral shock, M. Gosselin cites the case of a patient who was doing well, when he heard that his wife had become insane and was at the Salpetrière. Very soon afterward he began to shiver, and fell a victim to purulent infection.

M. Verneuil, the distinguished surgeon at Lariboisière, especially occupied himself with the consideration of the previous health of the patient. The influence of diseases, manifest or latent, of the kidneys and lungs, of drunkenness, miasm, etc., is constantly proved by the unfortunate results of the best conducted operations. M. Verneuil thinks that erysipelas more frequently occurs in individuals with the herpetic or arthritic diathesis.

M. Labat attached less importance to previous or coincident diseases, and agreed with M. Bourgade in the attention needed

for the local conditions of the wound. He lays down several rules as follows:

1. Never attempt to obtain immediate union except when the wound is shallow, the texture of the tissues uniform, the opposed surfaces can be maintained in contact as well as the edges, and the tissues have not been too profoundly bruised.

2. Carefully avoid all conditions which may lead to the alteration of the fluids, and their sojourn near the mouths of the veins.

3. Favor the draining of fluids by a tube or other means, establishing a canal from one end of the wound to the other.

4. Avoid the employment of all irritating substances, especially in regions abundantly provided with lymphatics.

5. In anfractuous wounds, fill up the anfractuosities with lint, so as to avoid the accumulation of fluids.

6. Preserve the limb as immovable as possible, and avoid too frequent dressings.

7. Abstain absolutely from the application of pure water on the wound; always use alcohol.

8. Whenever there is reason to fear purulent absorption, give ergotine in the dose of two to three grammes from the first day, and continue as long as the danger lasts, usually ten or twelve days.

A distinguished professor from Rome, M. Mazzoni, pointed out the necessity of isolating the surgical wards from those containing fever or tuberculous patients, a precaution hardly ever adopted in French hospitals; but at Naples, Professor Palasciano did not hesitate to tender his resignation when the attempt was made to approach a fever ward near that of his operated patients. M. Mazzoni asserted the comparative immunity of the Italian hospitals, even the maternities, from erysipelas and puerperal fever, in all cases except where the usual precautions to exclude patients affected with fevers or other contagious diseases, or with tuberculosis, are for some reason neglected.

Mr. Meric, of London, also claimed for the English hospitals the merit of great attention to this point, and ascribed to it much of the superior success of English surgeons in ovariotomy.

But sometimes the most lively debates of the entire Congress were excited by the question of syphilis, and its means of prevention by legal measures. With the exception of Dr. Drysdale of

London, and one other physician who wished to oppose moral education to the extension of the frightful evil, it was everywhere assumed that the only efficacious measures consisted in strict surveillance over prostitutes. In proof of the results obtained by this means, several members read elaborate memoirs. The first was sent by M. Wleminckx of Brussels, who pronounces that to be the best regulated of cities in this respect. All the registered public women are examined every three days, and punished if they fail to present themselves for examination. Upon the slightest suspicion of disease they are sent to the hospital. All physicians are forbidden to treat prostitutes at their houses. Rewards are offered to such women as present themselves regularly for examination. By means of these precautions, M. Wleminckx asserts that the number of syphilitic diseases has very considerably abated, and secondary and tertiary affections have nearly disappeared.

In addition to these measures applied to women, in military hospitals all syphilitic patients are rewarded if they will denounce the person from whom they have contracted the disease.

M. Crocq, also from Belgium, observed that these measures, so efficacious in the great cities, were neglected in small villages, which served as places of refuge for clandestine prostitution, and were indestructible foci of syphilis.

M. Rollet, in the name of the Imperial Society of Medicine at Lyons, advocated not only surveillance of the women, but of all men in situations where their conduct could be controlled, as soldiers, sailors, etc. In view especially of the terrible accidents recently occurring at a large glass factory, where the workmen being compelled to apply their mouths successively to the same tube, nearly all contracted the disease from one whose mouth was the seat of syphilitic ulceration, M. Rollet recommends the extension of this surveillance to the glass-blowers also.

M. Buchon made a report of the measures actually enforced in the French navy. Every sailor or soldier is submitted to an examination, previously to the arrival of the vessel in port, and none are permitted to go on shore without a certificate of perfect health. Same precautions before leaves of absence are granted. Thanks to this incessant surveillance, which although of ancient date has been especially vigorous since 1830, the navy department has greatly diminished the number of syphilitic patients admitted

into the hospitals. At Brest, where the hospital formerly always contained three hundred beds of such patients, the number has diminished to one hundred.

M. Le Fort presented some statistics concerning the actual state of prostitution in Paris. The total number of registered prostitutes is 3,851, of which 1,306 are distributed among one hundred and sixty-five houses—the rest are isolated. The amount of clandestine prostitution is enormous, but cannot be estimated. All soldiers treated for the disease are compelled to reveal its source, and the police pursue the woman. A certain number of girls are arrested every day for clandestine prostitution; among 13,818 of this category, 3,728 were found to be diseased, 1,131 were sent to St. Lazare, 7,217 reclaimed as minors by their families (!), 1,549 only were registered.

In six years 504,000 examinations have been made with the speculum upon prostitutes, and 3,720 contagious diseases have been thus discovered. This number is small in comparison to the number of examinations, but considerable in proportion to the number of prostitutes registered.

In spite of all this surveillance, as Mr. Drysdale of London remarked, syphilis is not less frequent in Paris than London, where prostitution receives no sanction from authoritative surveillance.

The question that really excited the Congress almost to a flame, was that of the possibility of preventing syphilis by inoculation. It is unnecessary to record the debate in which M. Ricord quite overbore M. Auzias-Turenne, who enthusiastically advocated such inoculation. Several very disastrous and even fatal diseases were reported by those who adhered to Ricord's doctrine, as the consequence of inoculation with the hard chancre. The discussion had no especial result.

Complementary sessions were held from time to time in the evening, in which various interesting subjects were suggested or debated. I have already over-passed my space, but must mention two communications of real curiosity.

The first is the exposition, by M. Brunetti, of a new method for preserving anatomical pieces. His preparations have been on exhibition at the Exposition, but the process hitherto has been kept secret. In an evening session, however, M. Brunetti revealed it; and, as I know from personal examination of his

preparations, the results are so admirable, that every one should be acquainted with the method.

Several operations are included; the washing of the piece, freeing it from fat, its tanning and desiccation.

To wash the piece, M. Brunetti passes a current of pure water through the blood-vessels and excreting canals; then alcohol to expel the water.

Then ether is made to replace the alcohol in order to dissolve the fat; this process requires several hours. The ether penetrates everywhere, and everywhere accomplishes its work thoroughly. At this point, the piece plunged in ether can be preserved indefinitely before proceeding to further operations.

Then tannin is dissolved in boiling distilled water, and this solution is passed into the blood-vessels, etc., after the ether has been driven out by a current of distilled water.

Then the piece is dried by being placed in a vase with a double bottom, and containing between the two, boiling water. By means of a reservoir where the air is compressed to about two atmospheres, and which communicates by a stopcock and a system of tubes, first with a vessel containing chloride of lime, then with another empty and heated, then with the vessels and excreting canals of the piece, M. Brunetti establishes a gaseous current which expels all the liquids. The operation is then finished, and the piece remains supple, light, with its natural size and relations, and all its solid histological elements. The most perfect microscopic slices may be made from the preparation.

The other invention, which is too good, or at least too striking, to be passed by in silence, is an instrument for *Somatoscopy*. This was presented by M. Millot, of Russia, and is designed to illuminate the cavities of the body, so as to render them transparent to the eye. The apparatus is composed of a glass tube containing a platinum wire curled up on itself, and communicating by copper stems with the two poles of an electric battery. When the current is passed, the platinum wire grows glowing white, and emits an intense light. This tube introduced into the stomach, vagina, or rectum of the cadaver, has enabled the observer to see by transparence the walls of the abdomen. M. Millot made some experiments upon animals before the Congress, but so far he has had no opportunity to test his apparatus on the

living subject. He hopes, however, by its means to bring great assistance to the diagnosis of tumors of the ovary, and even adherences, and also of calculi and tumors of the bladder.

P. C. M.

To the Editor of the Medical Record.

SIR—To-day celebrates the closure of the Exposition; to-morrow will witness the reopening of the Ecole de Médecine, and the beginning of the long medical year.

In Paris, the ceremonies of the year are reserved for its close as in Italy, while in England, as in Spain and Portugal, whatever solemnities are deemed fitting to dignify the old critical days of the scholastic season are observed at the moment of its recommencement. *L'Union Médicale*, of Paris, in reviewing the celebrations held at the different schools, greatly commends the simplicity of the English, who quietly assemble at the numerous "head-centres" of instruction, listen to a regulation address, and immediately set to work at their studies; whereas, in Madrid and Lisbon the affair is made a state occasion, honored by the presence of the king and highest public functionaries. It is pomp *versus* utility, says Dr. Simplice, and the contrast is manifest even in the themes chosen by the professors for the address. That of Mr. Graily Hewitt, for example, at the University College in London, was entitled, "The Therapeutic Utility of Alimentation," while the discourse of Professor Alonzo at Madrid was devoted to an elaborate exposition of "The Benefits of Instruction."

In Italy, the illustrious Professor Tommasi celebrated the close of the year of official instruction by a retrospective review of the most important clinical facts that have presented themselves to his observation since its commencement. M. Tommasi energetically insists on the sufficiency of clinical study to meet its own legitimate ends, and protests against the prevailing tendency to accept the ideas of Chomel and degrade it into a simple stepping-stone for pathological anatomy.

"Clinical study alone has established the causal relations between articular rheumatism and endocarditis, between alcoholism and arthritis on one side, and endo-arteritis on the other, between different species of constitutional infection and an increase in the volume of the spleen, between syphilis and certain special neoplasias of the connective tissue, between scarlatina and croupal inflammation of the pharynx and kidneys."

You remember that Continental physicians (not including, however, the French) are agreed to denominate all inflammations attended with fibrinous exudation croupal.

Epilepsy Depending upon Premature Ossification of the Cranial Sutures.

Among other interesting facts quoted from his clinic, Tommasi signalizes a case of epilepsy in a child, dependant upon premature ossification of the cranial sutures, especially the spino-occipital. This cause of epilepsy has been specially signalized by Virchow. In Tommasi's case, the disease was greatly ameliorated by the use of nitrate of silver, but it is difficult to imagine why.

Concerning Ptisans.

The use of ptisans is so widely spread in France, where cold-water drinking is considered at once an imbecility and a crime, that their selection becomes a matter of considerable importance. M. Miquel (de Tours) has just published some suggestions on the drinks most suitable in typhoid fever that might be not altogether useless at home. He proscribes all amylaceous and sugared mucilaginous drinks, especially those containing vegetable acids, and all fermentable preparations, on the ground that they increase the secretion of bile, and the confluence of the intestinal eruption. Therefore, instead of lemonade, currant jelly, gum and barley water, M. Maquel recommends infusions of linden and orange leaves, chamomile and mignonette; also water flavored with a few drops of coffee, tea, brandy, or rum. In preparing rice water, mixed with decoctions of poppy heads, the physician of Tours directs that the rice be not added to the decoction ready made, but only allowed to remain in contact with it long enough for the water to extract the astringent principle of its rind.

Dyspepsia and Its Treatment.

M. Malherbe, of Nantes, publishes some reflections on a subject calculated to interest the inmost heart of every American —on the treatment, namely, of dyspepsia. Considering that, in our favored land, all the blessings of liberty are impotent to save us from the grasp of this foul fiend, and that nearly every one of us has either had dyspepsia, or actually suffers from it, or is

destined to suffer in the future; no suggestions on the subject can afford to be lost. M. Malherbe strongly recommends the use of pure hydrochloric acid in all cases of the atonic form of the disease. He considers this substance to act as a stimulating tonic, which facilitates stomachal digestion by assisting to dissolve albuminous substances; by regulating the secretion of gastric juice; by remedying constipation in virtue of an exciting action on the intestine; finally, by a tonic action on the general economy. In various cachexias, even advanced tuberculosis, this medicine is found to render good service. I have myself had an opportunity of testing the truth of this observation, especially at Lariboissière, in the wards of M. Hérard. He is enabled, by means of this acid, to greatly relieve the various dyspeptic symptoms (among which frequent vomiting is not the least painful) which torment the last days of his numerous consumptive patients.

It is recommended to associate wine of quinquina, calumba, or rhubarb, and some preparation of opium with the hydrochloric acid. The following is the formula employed at the Hotel Dieu of Nantes:

Wine of Quinquina.........	100 grms.
Syrup Thebaic.............	30 "
Pure Hydrochlor. Acid.....	1 "
Mix.	

The dose is from two to six teaspoonfuls a day. To relieve the gastralgic pain to which many dyspeptics are martyrs, M. Miquel suggests the administration of a concentrated opiate combined with a bitter, which serves to correct its injurious effects. The following is his formula:

Syrup of Bitter Orange Peel,	āā q.s.
" Morphine........	
" Ether............	
Mix.	

Where the pain comes on principally *before* eating, it is advisable to administer a narcotic or etherized draught about a quarter of an hour before meals. It is M. Hérard's practice to give his patients ten drops of Sydenham laudanum immediately before, and one grm. of pepsine immediately after eating. This treatment entirely relieved the pain, and stopped the vomiting in

the case of a woman, who subsequently died from the effects of a diarrhœa maintained by deep tuberculous ulcerations of the intestines, and with whom the mucous membrane of the stomach presented the signs of such an intense arborescent injection, mingled with yellow and slate-colored spots, as really merited the title of gastritis.

Arsenic in Cerebral Congestions.

Therapeutics does not constitute at present the most fashionable subject of meditation in the medical world, so much the more, therefore, do I glean studiously all indications of experiment in this direction. M. Lisle had just read a note before the Academy on the advantages of arsenic in the treatment of intercurrent cerebral congestion among the insane. M. Lisle considers hallucinations to be, not a symptom, but a complication of insanity, and always dependent upon congestion, consequently always to be treated by arsenious acid. He claims to have cured 131 patients out of 193 by the use of this medicament, and to have markedly ameliorated the condition of twenty-nine others. If the facts cited by M. Lisle are trustworthy (and there is no reason to suppose they are not) they are in striking opposition with his theory. According to the analogy of its action in all other cases, arsenious acid should be considered as an eminent tonic of nutrition, regulating the life of the capillaries, perhaps, in several ways, but by no means tending to disgorge them of unseemly congestion. At the hospital Beaujon, M. Montard Martin told me that he employed arsenious acid with considerable success against cholera in the last epidemic, and there seemed reason to suppose that the capillary circulation of the surface was restored or stimulated by this potent drug in a manner to relieve the deadly visceral congestion. Moreover, as the editor of the *Montpellier Médical* remarks, it is far from proved that hallucinations are connected with congestion of the brain, a condition not indicated merely by some redness of the face and brilliancy of the eyes. According to the ideas of Luys in his recent brilliant researches into the minute anatomy of the cerebro-spinal system, hallucinations occur when the thalami optici instead of simply receiving impressions from without and irridating them to the periphery, set up an independent action, and originate impressions in the recesses of their own structure.

This might occur whether they were excited by congestion or their normal functions perturbed by anæmia.

For those who have not read M. Luys' book, it may be necessary to explain that many of his views on the structure of the brain are quite original. The particular theory to which I have just referred rests on another, purely anatomic, namely, that all sensitive fibres proceeding from the posterior and lateral columns of the spinal cord, are destined to terminate in the thalami optici, which constitutes the first receptacle and halting place for impressions received from the world without. Here the crude impressions are elaborated and ultimately radiated to the vesicular matter of the convolutions along the converging white fibres that apparently proceed from the surface to the base of the brain. These are not, as generally affirmed, the mere continuation of the fibres from the cord, but new ones, deriving their origin from the thalami optici themselves.

The Characters of Cerebral Softening.

In this connection it is natural to mention the essay in the *Archives de Médicine*, written by Proust, on softening of the brain. The dominant idea resulting from the researches of this distinguished young physician, is the separation of softening (*ramollissement*) both from encephalitis and hæmorrhage. Encephalitis determines a neoplasia, or is equivalent to it, precisely as inflammation generally involves the idea of exudation.

Hæmorrhage usually results from the rupture of a capillary aneurism. But *ramollissement* is a necrobiosis, essentially the same as gangrene of the limbs, and its phenomena only differ because the tissues involved are withdrawn from the action of the air. M. Proust, however, reserves the name necrobiosis for a molecular destruction of tissue, and to its destruction *en masse* assigns the term necrosis.

The death of the cerebral substance depends upon obstruction of the capillary circulation, however caused, whether by a thrombus, an embolus, stricture of the cerebral arteries, fatty degeneration of the capillaries, thrombus and phlebitis of the sinus, etc. In these cases, there may be produced either a condition of anæmia or of hyperæmia. If an obstacle to the circulation be situated in the sinus, there is always hyperæmia; obstacle in the capillaries occasions rather anæmia. The first pro-

duces the red, the second the white softening. Hyperæmia of the parts surrounding the focus of softening is easily explained by the collateral fluxion in branches of the vessel whose tension has been increased by the obstacle to the circulation. Hyperæmia of the centre of the infarctus is more difficult to account for, and M. Proust only suggests with some hesitation, that it may be due to some action on the part of the vaso-motor nerves, or the result of a functional alteration of the capillaries.

The white coloration is rarely observed, but occurs occasionally in cases of general cachexia, as in the case of cancerous patients.

The red coloration may be uniform, and is then more marked at the periphery; or spotted, and then results from little hæmorrhages arising from the rupture of a great number of capillaries.

Diminution of the consistence of the part is appreciable from the second day. The tissue has a trembling jelly-form appearance. Then the part becomes tumefied, it is softer, the furrows separating, the convolutions disappear, and by the third or fourth day it has become reduced to a diffluent paste.

The first phenomenon detected by the microscope is the appearance of fatty granulations in filtering the tissue. These have been observed at the end of twenty-two hours by Charcot, and thirty-six hours by Prévost and Cotard. Then granular bodies succeed to these fatty granulations and accompany them. According to Bouchard these granular bodies consist of agglomerations of fatty granulations.

Transformations speedily take place, corresponding to what has been described under the name of chronic softening, *plaques jaunes*, cellular infiltration. The coloring matter of the blood transudes through the walls of the capillaries; that already extravasated becomes converted into yellow granular amorphous masses, or else into reddish oblique rhomboidal crystals, the hæmatoidine of Virchow.

Later, a neoplastic effort sets up, and a tendency to cicatrization appears; the pia mater contracts adhesions with the bottom of the excavation that has been formed by resorption of disorganized tissue, and across this excavation are extended laminæ of connective tissue. The tissue surrounding the softened part becomes slightly indurated in virtue of a proliferation of nuclei and cells. This last is analogous to the formation

of an eliminating membrane around a patch of gangrene in a limb.

Prévost and Cotard, under the direction of Vulpian, have made a number of experiments on the production of foci of *ramollissement* by injection into the arteries of powder of lycopodium or tobacco. They succeeded in simultaneously provoking infarctus in the brain, the spleen, and the kidneys, were able to observe the apparition of fatty granulations and granular bodies, and determine the formation of *plaques jaunes*.

Although the opinions have been refuted which attached softening to inflammation, on account of a mistaken idea that the yellow degeneration consisted of pus, Proust admits that certain analogies exist between the two affections, especially in the formation of the zone of proliferation. Further, that it is impossible to say that hæmorrhage and softening exercise no mutual influence on each other; on the contrary, the first tends to destroy the tissue directly, and to cut off its nutrition, or, on the other hand, the second, by withdrawing from the capillary walls their normal support, predisposes them to yield to the pressure of the blood.

The influence of atheroma upon the production of hæmorrhage is of course unquestionable. I had an opportunity of observing a remarkable illustration in an autopsy recently performed at La Charité. The aorta was atheromatous from its base to a point below the cross; the carotids were sprinkled with atheromatous patches, but arrived at the brain, the internal carotid showed complete degeneration. The same was true of the branches of the basilar artery. In this brain, sections in any direction revealed a punctuated injection caused by rupture of the capillaries, and in the middle lobe of each side, adjoining the thalami optici, existed a small focus of hæmorrhage.

Truly yours,

P. C. M.

PARIS, Nov. 3, 1867.

Curious Nervous Phenomena.

To the Editor of the Medical Record.

SIR—The "sensation" of the week centres around the discussion of a remarkable circumstance occurring in the wards of M. Richet at Hotel Dieu. On the 23d of October, a woman

entered the service, who, falling against some pieces of sheet copper, had been wounded in the forearm by their sharp edge, in such a manner that the radial artery and median nerve had been completely divided. Notwithstanding this section, sensibility remained in the thumb, index and middle fingers, and the external border of the ring finger, all furnished by the median nerve; moreover, the peripheric extremity of this nerve was exquisitely sensitive to the touch of the pincers.

The fact has been examined and acknowledged by a number of distinguished physicians. There can be no doubt that the median was completely severed. But it is difficult to explain a phenomenon so contrary to the facts which form the basis of current physiological theories, according to which the peripheric extremity of a severed motor nerve preserves its motor power, and that of a sensitive nerve loses its sensibility. Dr. Fort, in the Union Médicale, endeavors to prove that the case is one of *recurrent* sensibility, like that described by Bernard as existing in the facial nerve. Irritation of this nerve excites pain, on account of its anastomoses with the trigeminus. In the same way, says Dr. Fort, we must infer from this fact itself that the radial and cubital nerves furnish anastomoses with the median, by which this latter is enabled to preserve its sensibility even after section.

This argument is rather *post factum*. Moreover, as observed by Dr. Reveillant in the *Gazette des Hôpitaux*, there is no analogy between the coupling of a sensitive and motor nerve in a single "nervous pair" (as in the case of the 5th and the 7th) and this supposed anastomosis between two sensitive nerves, which is justified by no precedent whatever. Dr. Reveillant maintains, that since the grand palmar nerve is more deeply situated than the median at the wrist on the level of the wound, since it is united to the median only by a loose cellular tissue which permits great mobility, since the median was torn, instead of being distinctly cut, and the wound was deepest on the radial border of the wrist—in view of these considerations it is probable that the great palmar nerve was not divided, as at first supposed, and the recurrent sensibility was due to its presence in the parts furnished by the median. But this explanation, though hypothetically satisfactory for the sensibility remaining in the hand, renders no account of that preserved in the peripheric extremity of the median, since the palmar branch is given off from this latter

nerve at a point above the situation of the wound, and afterward has no connection with the median. Dr. Richet has not yet spoken. His detailed description of the case is presently expected, and may throw light on this vexed question.

Experiments upon Criminals.

At the naval medical school of Brest, M. le professeur Duval has pursued some physiological researches in a direction that continually tends to become restricted. If the humanitarian tendencies of the age prevail, capital punishment will be abolished and physiologists for ever deprived of the bodies of criminals as material for experiments. In view of this unfortunate contingency, all experiments actually performed are invested with a double interest, on account of the possibility that they may be the last permitted in civilized countries.

The following is a *résumé* of the results obtained by M. Duval, in galvanization of the different apparatus of the bodies of criminals, within five or six minutes after their execution.

Nervous System.—Galvanization of the motor ocular nerve, at its point of immersion in the cavernous sinus, caused instant contraction of the dilated pupil. In two subjects reflex movements were excited by a brusque tap on the hands or feet. The contractions of the deltoid, brachial, biceps, anterior tibial and gastro-nervous muscles were especially evident. In the case of a slight irritation of the surface, exciting contraction of the subjacent muscles, the action was evidently reflex: where a smart percussion had been practised, M. Duval supposes that the muscular fibre had been directly excited, independent of the nerves. This experiment confirms Schiff's refutation of the theory that warm-blooded animals could not exhibit reflex movements after decapitation.

Digestive Apparatus.—The stomach and small intestines continued to exhibit peristaltic movements for several minutes. The stomach was filled with food, and in the midst of digestion, but none of its contents escaped at the orifices after removal of the organ from the abdomen, so efficient was the contraction of the sphincters. A remarkable prominence of the solitary closed follicles of the ilium was observed. This fact is interesting, on account of a theory recently proposed, which considers such prominence to be a characteristic lesion of cholera.

Circulatory Apparatus.—The primitive carotids were divided, and their extremities were seen to rise at regular intervals, elongate beyond the level of the wound, and then subside; at each impulse a small quantity of frothy vermilion blood escaped.

On the same two subjects, the thorax was opened seven minutes after death, and the heart found to be beating within the pericardium. Upon incision of this membrane, the following succession of phenomena was observed. At the beginning of each movement the auricular appendix was suddenly raised, and distanced from the aorta, then fell as abruptly into its primitive position. In rising, the appendix lengthened, and the indentures of the circumferences parated like the fingers of an outstretched hand. At the same time with this erection of the appendix, occurred an *expansion* of the auricle, as if it were distended by an efflux of liquid. The contraction of the ventricles followed that of the auricles in less than the fifth of a second. These cavities were shortened in all their diameters, their surface became furrowed, they contracted together in perfect synchronism.

After cessation of the spontaneous beatings of the heart, the movements were renewed by the application of galvanism, first to the organ itself, afterward to the spinal cord.

M. Duval found that a moderate degree of contractility existed in the walls of the aorta, which, irritated by the insertion of a finger, were found to slightly press upon it.

Respiratory Apparatus.—Upon galvanization of the external or internal intercostal muscles, or of both together, the under rib was raised and pushed outward. M. Duval concludes that both these muscles, concerning which, from the time of Haller and Hamberger, there has been so much discussion, are inspiratory. M. Duchenne (de Boulogne) impresses this fact into his service to prove the same theory.

Atrophy of Muscles of Trunk and Limbs.

I am not sure whether I have mentioned the lecture delivered by Duchenne upon a patient in the wards of M. Bouillaud, affected with atrophy of nearly all the external muscles of the trunk and limbs. The intercostal muscles were entirely wasted, and the chest is flattened in a remarkable manner. The respiration is performed by the diaphragm. M. Duchenne

remarked, that since the thorax contracted, as in expiration, as soon as the intercostal muscles became powerless, it was just to infer that in health they antagonized this contraction; that is, opposed the action of the expiratory muscles. It seems to me evident, however, that the case in question proved that they exerted such antagonism in virtue of their *tonicity*, not at all on account of the intermittent contraction during inspiration. The experiments of M. Duval are no more conclusive, for although a muscular fibre when galvanized should elevate a rib to which its fibres were attached, we are not thence to infer that it contracts habitually during life, or therefore that it directly elevates the ribs during inspiration.

P. C. M.

To the Editor of the Medical Record.

Sir—Before the Academy of Sciences, M. Sappey presented a note announcing the existence of NERVI NERVORUM, or nervous filaments, in the neurilemma of nerves, analogous to the vascular ramifications in the coats of blood-vessels. The distinguished anatomist has followed these filaments as far as the sheaths enveloping the secondary trunks of nerves, but they are never found in the envelope of primitive fascicules. The internal envelope of the optic nerve receives no nervous filament. The external, on the contrary, receives a number from the ciliary nerves. This external sheath is also remarkable for the abundance of elastic fibres which enter into its composition. It therefore differs notably from both the sclerotic and the dura mater, which are deficient both in nervous filaments and elastic fibres.

The Curvature of the Spine and the Ossification of the Ribs.

M. Sappey is the Chef des Travaux Anatomiques, and has just reopened his popular course at the Ecole Pratique. At a lecture at which I had the pleasure of "assisting" the other day, M. Sappey referred to two points which had been the object of some recent personal researches, and may not, therefore, be well known to you. One was an explanation of the curvatures of the vertebral column by the obliquity, in the cervical region of the intervertebral disks, in the dorsal, of the bodies of the vertebræ, and in the lumbar, of both bodies and disks. Hirschfeld had

attributed these curvatures to the action of the yellow ligament, and declared that they were destroyed by its section. M. Sappey had repeated the experiment, and found this assertion incorrect—the curvatures persisting.

The other point referred to was the ossification of the ribs. According to M. Sappey, the ribs, like the bones of the cranium and the face, pass through no cartilaginous stage, but a thread of osseous substance is found to be formed directly in the midst of the original "mucous" mass.

The Function of the Vaso-Motor Nerves.

Drs. Eulenbers and Landois have published a series of articles upon the function of the vaso motor nerves, and upon the rôle they seem to play in a certain intermittent ophthalmia. Griesinger considers this affection to be a form of latent intermittent fever, as a neuralgia of the eye more or less severe, accompanied by congestion more or less intense. It is nearly always unilateral, and consists in an intense hyperæmia of the eye, with photophobia, suffusion, contraction of the pupil, and often œdema of the iris. When the disease is of long standing, it may terminate in chronic ophthalmia, or in atrophy of the bulb. Griesinger considered the presence of neuralgia essential to characterize this form of ophthalmia, but Mannhardt has reported a case where this symptom was entirely wanting.

A man thirty-six years old was suddenly attacked at nine o'clock in the morning with an acute catarrhal conjunctivitis. Intense redness and swelling of the palpebral and bulbar conjunctiva, abundant flow of tears mixed with mucous flocculi. A collyrium of acetate of lead was ordered. The next morning there was no trace of the affection but it returned with as much intensity as ever at two o'clock in the afternoon. The same collyrium was employed, and the inflammation again disappeared, to reappear the fifth day between nine and two o'clock. Small doses of quinine were then ordered. An access of moderate intensity occurred the seventh day, but from the ninth the disease did not return.

It may therefore be admitted that the vaso-motor filaments of the trigeminus may be affected independently of the sensitive fibres and that intermittent ophthalmia may exist uncomplicated by neuralgia.

Curious Phenomena Presented by Primitive Syphilitic Indurations.

In the Archives of Medicine for November, M. Fournier, a distinguished agrégé of the faculty, has called attention to certain curious phenomena occasionally presented by primitive syphilitic indurations. The first and most interesting is an ulceration of the cicatrized chancre. The second is the softening of the central and deep portions of the induration, and its progressive elimination in the form of a purulent detritus. The third phenomenon relates to the production of secondary indurations, resulting from the primitive affection, and occurring in the neighborhood of the initial chancre.

In the first case, the physician may have had to deal with a chancre which has accomplished its different phases with perfect regularity, and has cicatrized in a perfectly satisfactory and apparently definite manner. Under certain circumstances (of which an unusual abundance of the induration seems the most characteristic) this cicatrix is found to open, ulcerate, and erode in various points;—a new wound is thus formed on the surface of the induration, which sometimes excavates its entire extent. M. Fournier has observed this secondary ulceration to be repeated three times on the same base. In the cases in question the rupture of the chancre is entirely spontaneous.

Although in the greater number of cases the ulceration takes place on chancres in which the induration is excessive, it may also occur when this is of only medium intensity. The ulcer is formed from the eighth and fifteenth days after the cicatrization. Sometimes it is quite superficial, a simple erosion; sometimes it affects the excavated form. A sanguinolent rather than purulent liquid is secreted by the wound. The ulcer generally heals with remarkable rapidity, and, although sometimes alarming from its extensive and ragged aspect, it is in reality benign. Even when assuming a phagedenic form, it readily heals with only an application of dry lint.

The conversion of the cicatrized chancre into an abscess, much more rarely occurs than its ulceration. In this case, also, the cicatrix has been regularly formed, and the induration is generally excessive. Presently the centre of the mass is felt to be softened, and a little later a small opening is discovered, through which is eliminated a yellowish sanguinolent liquid, puriform

rather than purulent. As many as six openings have been observed, each leading by a curiously formed little passage into a central focus of softening. The integrity of the outer layers of the mass is preserved.

It is evident that the ulcer and the abscess are really analogous lesions, each producing a liquefaction and consecutive elimination of the pathological tissue of the induration. May not the exaggeration of this latter, which has been found so generally to coincide with the lesions, be indirectly their cause, on account of opposing greater difficulty to the ordinary process of absorption?

The secondary indurations may ulcerate, and assume the aspect of primitive hard chancres. M. Fournier thinks that it is on account of cases of this kind that Babington had been led to maintain that syphilitic induration preceded ulceration. An opinion that M. Fournier has no hesitation in pronouncing erroneous, if only on account of the difficulty of diagnosis between initial chancre and herpes.

Operations for Naso-Pharyngeal Polypus.

The *Gazette des Hôpitaux* contains an account of an interesting operation practised at the Hotel Dieu of Clermont Ferrand, for a naso-pharyngeal polypus. The patient was a boy of eighteen, extremely diminutive and fragile. The polypus had apparently existed eighteen months. The right cheek was but slightly deformed, but the difficulty of speaking, and the embarrassment of the respiration, forcing the patient to keep the mouth partially open, indicated the existence of a material obstacle to the entrance of air.

The soft palate was pushed forward by a hard, resistant, bright-red tumor, whose lower border projected below the uvula, its adherent edge mounted in the pharynx. The nostril of the same side was obstructed by a fleshy mass, evidently only an expansion of the guttural tumor.

The finger, introduced between the cheek and the alveolar arcade of the upper maxilla, distinguished a small tubercle which corresponded to the exterior tumor. The point of insertion was difficult to determine; nevertheless it seemed probable that the tumor adhered rather to the pharynx than to the nasal fossa.

After some delays, during which the respiration became more and more embarrassed, M. Fleury determined to afford the

patient the only chance for life that remained, by practising the resection of a portion of the maxilla, and thus extracting the tumor. The patient being under the influence of chloroform, an oblique incision was made from the commissure of the lips to the external angle of the orbit. Only one artery required ligature. The upper flap was dissected to a considerable distance, then a chain saw introduced by means of a curved needle into the spheno-maxillary cleft, to separate the maxilla from the malar bone. The second lateral incisor tooth was extracted, and the *cisailles*, introduced into the mouth and right nostril, easily divided the palatine vault—a section practised with scissors separated the apophysis of the maxilla; it then was only necessary to apply the blade of the same instrument underneath the orbit and exercise a light pressure, to loosen the bone. The soft parts uniting it to the subjacent tissues were detached with curved scissors, and the bone then easily removed. The nasal fossa and zygomatic cavity were thus laid open, and the opening, though smaller than if the maxilla had been entirely removed, was sufficiently large to give passage to the tumor, and the risk of deformity was much less than would have been incurred by the other operation. The polypus being discovered, was much larger than had been supposed, since it occupied the cavities of the face. As much as practicable, M. Fleury endeavored to enucleate the tumor. The pedicle was implanted by a large and resistant base in the upper part of the pharynx. It was fortunately but slightly vascular, and the slight hæmorrhage following its division was easily arrested by the cautery. The tumor weighed 112 grains, and was nearly entirely fibrous.

The second day after the operation, inflammation of considerable intensity set up around the wound, and destroyed all hope of union by first intention. The following days a suspicious odor escaped from the mouth and nostrils, which was partially neutralized by lotions of chlorinated soda. The patient was extremely feeble, and a fatal termination was dreaded. However, after some days of uncertainty, the lips of the wound, which had opened, assumed a better appearance and became covered with healthy granulations, the patient was able to rise, to eat, and from that moment the convalescence was assured.

The inflammation was attributed in part to the use of the

actual cautery. The wound was dressed with lint steeped in camphorated alcohol.

This is the third case in which M. Fleury has saved a patient from inevitable death, by boldly venturing on this formidable operation on the maxilla.

Among the clinics recommenced with the reopening of the year, that of M. Gosselin, who has succeeded the lamented Velpeau at La Charité, is not the least interesting. The inaugural lesson was divided into three parts. In the first the new professor paid a just tribute to the memory of his predecessor. In the second, he traced a rapid sketch of the history of this famous hospital, founded by Marie de' Medici 260 years ago. During 150 years there was no clinic, and the names of no surgeons have come down to us. The first illustrious clinician of the Charité was Desault, who instituted the *concours* for the position of assistant surgeon. Deschamps was the first who obtained this title, and also the first who availed himself in his scientific writings of observations taken among his hospital patients. Until then, observations had always been collected from the writer's private clientèle. Of all the writings of Deschamps, the best known are his "Observations on the Ligature of the Principal Arteries of the Extremities, on Account of Wounds or Aneurisms." Boyer succeeded Deschamps, and Roux followed Boyer; finally Velpeau took the place of Roux, when the latter succeeded to Dupuytren at Hotel Dieu.

The third part of the lesson was devoted to two patients in the wards, one with a fracture of the wrist, the other with an encysted encephaloid tumor at the internal and lower part of the thigh. Space does not permit me to quote at length M. Gosselin's remarks, further than that, in the latter case, he considered that amputation would not prevent a return of the tumor, but would prolong life.

P. C. M.

To the Editor of the Medical Record.

SIR—You will remember, no doubt, that at the International Congress the theme which opened the debates, and which occupied a most prominent place in the discussions, was the apparently exhausted subject of tuberculosis. M. Villemin has just offered to the Academy the treatise of which his remarks

at the Congress were the abstract, the exposition, and the defence. The researches of this ingenious experimentalist have led him to conclusions differing so widely from those generally adopted, as to excite the curiosity, applause, or indignation of every defender of the medical faith. Hence the report of M. Colin on Villemin's book has been followed by an able and lively discussion of unexpected interest in connection with a disease which had, so to speak, fallen into disuse, and whose victims were regarded as useless incumbrances of the clinical wards in the hospitals.

Inoculability of Tubercle.

The novelty of Villemin's views is manifested on three important points. 1st. Denying the existence of epithelium in the pulmonary alveoli, the physician of Vâl-de-Grace contradicts Reinhardt's assurance that the cheesy masses occupy the air-cells, and ascribes to them the same origin and seat as that generally admitted for the gray granulations, viz. the connective tissue between the alveoli, and around the blood-vessels. These masses are the result of fatty degeneration of the plasmatic elements of the connective tissue, whose proliferation has given rise to the nuclei and small cells characteristic of the centre of the gray tubercle. These last elements invariably degenerate, but not unfrequently the large connective cells on the periphery of the granulation are also invaded while yet undergoing the process of multiplication. In this respect therefore, Villemin returns squarely to the views of Laennec, who regarded the cheesy masses as softened tubercles.

2d. The most remarkable part of the *Etudes sur la Tuberculose* is that which relates to the experiments on inoculation of tuberculous matter, some of whose results were submitted to the Academy in 1865. Villemin was induced to make these experiments, by observing the histological resemblance of the elements of the miliary tubercle with those of the tubercle of syphilis or glanders. Since they were specific and inoculable, he inferred that the gray granulation might be so as well. The second half of this supposition has been fully confirmed. An immense number of experiments have been performed upon rabbits, by inserting into the subcutaneous cellular tissue fragments of pulmonary tubercles, and in nearly all cases the injection

was followed by an eruption of miliary granulations in the lungs, and by the constitutional symptoms of tuberculosis, to which, after awhile, the animals succumbed.

3d. Upon the success of these experiments, the first of any consequence that have ever been made in this direction, M. Villemin bases an entirely new theory of tuberculosis. He claims that what is inoculable must be specific; that tuberculosis belongs, in its character of specificity, to a family of diseases, depending on the substantial introduction into the system of a peculiar animal virus. It is, in short, a definite, virulent, contagious disease, like syphilis and glanders; and the histological similarity between the tumors in the three cases is justified, so to speak, by their family or generic affinity.

Now, as to the reality of the results obtained by M. Villemin in his experiments, there can be no doubt. They have been repeated with equal success by M. Colin, who reports the new treatise to the Academy; their accuracy is acknowledged by M. Cornil, from whose party Villemin has made such a frightful secession. Bouchard, in his review in the *Gazette Hebdomadaire*, and Chauffard and Pidoux in their speeches at the Academy, all admit this striking and unexpected discovery—viz. that tuberculosis, anatomically and clinically characteristic, may be communicated to rabbits and guinea-pigs by inoculation from the tubercles of cattle or human beings.

But the inferences adopted by M. Villemin are extremely contestable and contested, as M. Chauffard by an anatomical, M. Pidoux by a general analysis, successfully disproved the pretended virulence of the tubercular deposit. Chauffard points out that inoculations of specific animal poisons, as those of syphilis, small-pox, hydrophobia, etc., are first made with fluids containing no morphological elements or special characteristics. But Villemin's favorite experiments consisted in *grafting* a definite structure upon the organism. When this structure, sown on soil rendered congenial by the presence of lymphatics, develops itself and excites the surrounding tissues to similar proliferation, it does so in virtue of the laws of development of tumors, which, according to Virchow, depend on the foundation of a tissue by elements coming from another tissue.

2d. A definite period of incubation is essential to the process of virulent inoculation; after which appears local trouble, speedily

followed by general symptoms of infection. But M. Colin shows that in the experiments there is no such incubation, and no reproduction of the tubercle on the place where it was inoculated. The tumefaction observed there results from the resistance offered by the tubercle to the dissolving action of altered pus, on account of which some of the matter originally introduced may often be found on the same spot six weeks afterwards. M. Pidoux declares that the tubercle, placed at the base of the scale of heteroplastic formations, multiplying like all inferior organisms, dying speedily, and infecting the locality with products of decomposition, is in the highest degree incapable of the incubating force, the latent and refractory vitality characteristic of virus.

3d. The gray granulations are possessed of no exclusive power of infection. Villemin himself has produced an eruption of miliary tubercles by inoculation of the cheesy detritus, and even admits this to be the most favorable for the experiment. It is on this fact that he bases the revival of the doctrine of identity between the cheesy mass and the crude tubercle. M. Colin has successfully inoculated various animals, rabbits, guinea-pigs, lambs, calves, and dogs, not only with the cheesy deposit, but with the hard cretaceous tubercle from the lungs of oxen. Clarck has succeeded with ordinary pus; and Empis with pus from puerperal peritonitis, from the surface of Peyer's patches ulcerated in typhoid fever, and from suppurating fibrinous pneumonia. Finally, Lebert has experimented with mineral substances, as mercury and carbon. In all these cases a crop of perfectly characteristic gray, hard, semi-transparent tubercles was obtained in the lungs of the animals submitted to the experiment. It is impossible to imagine a more complete demonstration of the *common* and non-specific origin of the crude tubercle.

4th. M. Colin, in his report, follows step by step the consequences of the inoculations, and shows that for a long time they are purely local. The disease is communicated, not by the general infection of the whole system, but by the implantation of a thorn, whose irritation gradually extends and involves the lungs. The focus of inoculation is presently surrounded by radiating white lines, formed by lymphatic vessels engorged with foreign matters. The ganglia in which these vessels terminate engorge themselves also, and become filled with tubercular granulations, and others in their turn. The ganglia not found on

the route traversed by the morbid vessels, remain perfectly sound. From the lymphatic system, the tuberculous matter gains the central organs, probably by the route of the circulation, and is thus gradually deposited in the lungs, liver, spleen, and kidneys. This evolution is exactly that of the gradual propagation of a local evil, not the simultaneous impregnation of the entire organism by a virulent agent.

Colin is so impressed by this local character of the disease artifically produced, that he jumps to the conclusion that natural phthisis is also the result of local mischief, resulting from one or more tubercles that have at some time been introduced into the economy, and after remaining latent for an indefinite period, are suddenly awakened to activity. But this supposition is entirely gratuitous.

5th. M. Pidoux dwells upon Villemin's admission that the yellow tuberculous matter is more active than the gray tubercle. If we assume (which, as we shall presently see, is conceding too much) that this cheesy deposit be in fact a degeneration of the tubercle, a striking contrast becomes apparent between the tubercle and virus. This is more active in its first stages than at the period of its degeneration.

6th. Pidoux also observes that the similarity between the histological structure of the tuberculous tumor and of syphilis and glanders, is really an argument *against* the virulent character of the first affection. For at the moment that the other two diseases have resulted in tumors, they have ceased to be virulent, and have passed into a state of diathesis. Inoculation from those tumors will give rise to neither glanders nor syphilis. Hence this grand foundation-stone of the new theory, and this initial observation of M. Villemin's researches, is wrenched from him, and turned most ingeniously into a powerful argument against his cause.

From this *critique* it appears that inoculation of tuberculous or other matter acts on the lungs (whither it has been brought by the blood, and arrested by the fine network of capillaries), by irritation of the plasmatic cells of the connective tissue. These, proliferating, give rise to the small elements, which, closely crowded together, constitute the gray tubercle. The process is closely analogous to the proliferation of inflammation, which also results in the formation of the small cells and nuclei of pus,

which cannot by their form be distinguished from those of the crude tubercle. The difference consists—first, in the intercellular substance, liquid in pus, finely granular in the tubercle; second, in that the fatty degeneration results in cheesy masses for the tubercle, while the pus remains liquid.

In all cases of artificially induced disease the irritation comes, of course, from without, and may be called local. This may occur also, as Pidoux observes, in acquired phthisis, especially in that of miners and others constantly exposed to direct irritation of the lungs. But in constitutional and hereditary consumption the tubercular process is to be regarded simply as the final stage to which all irritative processes in weakly subjects naturally tend to degenerate.

"The impoverishment of the field of nutrition is the first condition of tuberculosis," says Pidoux; "the occurrence of some irritation the second. Nothing is more susceptible of irritation than weakness, nothing so ready to degenerate." Again (for I quote willingly from this able and brilliant discourse in which the old colleague of Trousseau so well justifies his reputation),

tuberculosis is the constitutional alteration, the characteristic and organic heteroplasia of the lymphatic apparatus, the fundamental apparatus of nutrition. This is attacked in the connective tissue, which constitutes its base. When this tissue sustains what Hunter calls the *stimulus of imperfection*, it is excited to proliferations, imperfect, sickly, of an extremely ephemeral vitality, born in fact but to die; such is the tubercle.

Chauffard ingeniously suggests that the ease with which the tubercle may be inoculated depends precisely upon the poverty of its organization. The fecundating tissue, to refer again to Virchow's idea and expression, would need to make less effort to assimilate another to a feebly organized structure, than to one complex, rich, and characteristic; and from this point of view the inoculation of tubercle is more easily comprehensible than that of cancer.

But the existence of an internal cause for tuberculosis, of a diathesis and of hereditary tendency, is denied by M. Villemin, and it is this denial that constitutes the fame of his heresy. He is as frank a believer in the contagious origin of phthisis as a doctor of the sixteenth century, or an Italian or Spanish peasant of the present day. For him *réunions* of consumptives constitute

more deadly foci of infection than cholera hospitals, and the lives of the patients sent to Nice and Cannes are terribly shortened by the atmosphere impregnated with emanations from the reeking lungs of their fellow-sufferers.

Upon this point especially does M. Pidoux attack the innovator. He charges him, not unfairly, with having entirely neglected the clinical study of tuberculosis in his absorption in its anatomical pathology. Examination of lungs after death is extremely useful as a means of ascertaining the results of disease and many of its processes, but must usually be incompetent to determine its cause. And it is glaringly illogical to conclude that because in a given case a disease has been artificially produced by inoculation of certain substances from without, therefore all spontaneous cases of the affection depended on the same mechanism. As well argue that capillary bronchitis could only be caused by injection of snuff into the trachea.

Yet precisely to this complexion has come M. Villemin. He formally denies the possibility of any spontaneous alteration of the organism, and insists that all disease must result from the infliction of exterior agents. Thus, from the reformed basis of phthisis, he dares attempt the reform of all pathology. But, as Pidoux observes, all exterior agents would be without effect were it not for a susceptibility on the part of the organism to be affected, which capacity itself constitutes a spontaneity and individuality.

In the meantime M. Pidoux is far from claiming for phthisis that overwhelmingly predestined character with which it is popularly associated. He admits that about one-sixth of all consumptive patients contract the disease in virtue of direct hereditary influence, but that many others are predisposed on account of *transformed* hereditary influence; arthritism, herpetism syphilis, or scrofula in the parents, tending less to reproduce themselves in the children than to occasion pulmonary tuberculosis.

Similarly, phthisis springs up in the wake of many diseases, which at their height are directly antagonistic to it. This is the case especially with arthritism, whose remains seem as it were to enrich the soil of the economy, and prepare it for phthisis. Such transformation takes place with the same individual. But in hereditary transmission M. Pidoux announces as a sufficiently

definite law, the progress of capital or initial diseases, first towards mixed diseases, then those that are ultimate or organic. Thus arthritism, scrofula, syphilis, capital diseases, change into herpetism, neuroses, neuralgias, catarrhs, etc., or mixed diseases; and these in turn degenerate into organic diseases, as tuberculosis, cancer, epilepsy, and incurable degenerations of the nervous centres. M. Villemin entirely denies any connection between scrofula and tuberculosis, which is the more singular as he cannot ignore the readiness with which the lymphatic glands become the seat of cheesy degenerations, such as he identifies with tubercle. It is precisely because he is forced to admit the multiple origin of scrofula, that he seeks to separate it from tuberculosis, of which the specific character must be preserved at all hazards. Pidoux, fully conceding the difference between the two diseases, justly insists on their frequent etiological connections. Scrofula is the initial chronic disease, often superficial, curable, and not an organic malady, although capable of becoming so. Tuberculosis, and especially pulmonary phthisis, are ultimate and organic diseases, too often the final stage of non-tuberculous affection. It is quite as necessary for scrofula as for arthritism or syphilis to degenerate before it gives rise to tuberculosis; patients with scrofulous ulcers are not consumptive, but lymphatic constitutions, delicate, nervous, "civilized," fall an easy prey to phthisis, often because of their escape from the external manifestation of the disease.

Finally, it is unquestionable that a number of persons untainted by hereditary vice of constitution, and uninjured by previous disease, fall victims to consumption on account of exposure to cold, to want, to privations. Pidoux seems to imply that in these cases the disease commences in bronchitis, determining proliferations of alveolar epithelium, which degenerates into cheesy masses, which ultimately excite a crop of tubercles by irritation of the connective tissue. Pidoux, therefore, fully admits the German distinction between pneumonic phthisis and granular phthisis; the last being constitutional, the first accidental, and often extremely rapid. Bouchard adheres also to this view, and sums up the differences between the gray tubercle and yellow masses, which nearly, if not quite, demonstrate their independence of origin. In the first stages of the "cheesy pneumonia," as the yellow masses are called by the new-school pathologists,

the alveoli are found partially filled with large pavement cells, which, in spite of M. Villemin's assertions, reveal their epithelial character by being more or less soldered together. The alveoli also contain serous exudation. On the contrary, the first stage of the granulation consists of a mass of small spheric cells, $0^{m\ m}$, 008 in diameter, with the nucleus filling the cavity almost completely, closely pressed against each other. This mass is situated not in the alveoli, but in the connective tissue at the bifurcation of the blood-vessels. In the second stage of catarrhal pneumonia the liquid is absorbed, the anatomical elements accumulate and become infiltrated with fat, and presently the alveoli are rendered entirely impermeable to the air, and offer on section a smooth level surface uniformly gray and homogeneous. The granulations also submit to the cheesy degeneration, but for a long time retain their form, so that different zones are distinguishable in their mass.

Villemin's second thesis, therefore, which constitutes his remarkable discovery, is everywhere confirmed, and it is an acquired fact that it is possible to produce pulmonary phthisis artificially, by inoculation with tubercular deposit, or with the products of the pneumonia accompanying, determining, or determined by that deposit. But his first theory, that identifies the tubercle and the pneumonia, and his third, which would make of phthisis a specific virulent disease, seem to be sufficiently refutable and refuted.

I only mention in passing the theory of contagion, for that is confessedly based on no clinical facts, but those dubious ones that have already done service for this theory. M. Villemin seems to infer that the contagion of phthisis must be a necessary consequence of its inoculability. But this is evidently a strained conclusion, since the conceivable mode of transmission between human beings must be widely different from that practised by Villemin on his rabbits. The hateful practical consequences of this doctrine of contagion may justify, perhaps, a partiality for M. Pidoux's vehement denunciation of its possibility. I have ventured to devote so much space to this discussion (of which I have endeavored to render the substance, but have been unable to transfer the zest and animation), because it is one of the most important that has taken place in Paris for some time. The daily urgent practical need of interest in pulmonary consumption

is so great that even a scientific vagary that should rouse the flagging attention to a worn-out theme, would be of value. This book of M. Villemin's, however, is no vagary or frivolity, as you may judge from the elaborate report that has been made of it to the Academy, and from the spirited debate to which it has given rise. But, in spite of its ability, and the great interest of the experimental researches, it is a reaction in a sorrowful direction. All hopes of curing phthisis depend upon its nature as a general disease of common origin; and the theory which tends to make it specific condemns the physician to inertia, or the vain revival of forgotten specific antidotes. It is with pleasure, therefore, that I find that M. Villemin's arguments are less sound than they are brilliant, imposing, and endowed with the charm of novelty. P. C. M.

PARIS, January 2, 1868.

Concerning Aphasia.

To the Editor of the Medical Record.

SIR—Americans are bound by every natural principle to oppose the tendency to centralization, which, like a dose of hashish, serves to concentrate the consciousness of entire France upon its head, Paris. We may most profitably and agreeably fulfil this duty by giving a hospitable reception to the medical and scientific news so richly furnished by the provinces.

One of the most interesting memoirs that have appeared during the past fortnight was read at the Imperial Society of Medicine, at Marseilles, by Dr. Fabre, wherein are discussed, with much clearness and originality, three problems concerning aphasia, a disease whose symptoms and pathology are so remarkable and mysterious.

A fourfold division is made of the disease. In the first variety or degree the patient forgets words; in the second, he loses voluntary control over their formation; in the third, he ceases to understand their meaning; finally, all these conditions may coexist in the most complex form of aphasia.

The loss of the faculty for written language, which is so remarkable a secondary phenomenon of aphasia, also exists in four degrees. In the first, the patient loses all recollection of written letters or words, but is able perfectly well to copy models

placed before him. In the second, he is unable to write, even when understanding what he wishes to transcribe. In the third case, he has lost the faculty of reading; and if he tries to write, although he succeeds sometimes in forming the letters well, he cannot co-ordinate them into words. Finally, all understanding of written or spoken language may have been completely abolished, while the rest of the intellectual faculties remain completely intact.

It is remarkable that when aphasic patients are unable to express their wishes, either by words or gestures, they sometimes succeed in giving utterance to their feelings. Thus, a lively sense of gratitude inspired a patient at Hotel Dieu to utter the only word that he pronounced in the ward; he said "*merci*" to the *réligieuse* who was taking care of him.

In the first class, there are various degrees of forgetfulness. Some patients forget proper names, or the greater number of substantives, and express their meaning by circumlocution. Thus, instead of asking for a pen, they demand something to write with. Others cannot construct a complete sentence. In the second class the patients pronounce words differently from what they intend; and although conscious of their mistake, and irritated by it, they are unable to rectify it. After this simple perversion of language comes real impotence; the patients express all their meaning with the same word, or even syllable, often utterly devoid of sense. Thus a patient of Trousseau's always repeated the word *consisi*, and the syllable *tan* constituted the entire vocabulary of a patient of Broca's. With these patients the movements of the tongue are perfectly free, and there is not a trace of glosso-labio-pharyngeal paralysis.

In the third category, the functional trouble is less grave, as regards the mechanism of speech, and more serious in respect to intellectual disorder. The patients cease to understand the meaning of their own words. and when they wish to say one thing, express a meaning directly the opposite. Thus a lady receiving visitors, addressed them in terms of gross insult, supposing that she simply invited them to be seated.

In the most complete cases of aphasia, from the testimony of certain physicians who have been affected by it and recovered, the intelligence is still perfectly intact. Thus Rostan observed his own case, and mentally prepared a clinical lecture upon it.

Lordah, and Dr. Spalding of Berlin, had a similar experience. Whatever difficulty is encountered in intellectual exertion is not a cause of the aphasia, but a result, on account of the loss of signs necessary to give precision and support to thought.

M. Fabre enters at length into the discussion of the anatomical locality for the lesion in aphasia. He inclines entirely to the opinion that, in the majority of cases, the left frontal lobe is the seat of the disease. Four or five cases have, however, been reported, in which a destruction of both the anterior lobes was unaccompanied by any symptom of aphasia. In these cases, however, the posterior part of the lobes was nearly intact. Moreover, M. Fabre suggests, although the faculty of speech be specially localized in this part of the brain, that in case of need, other portions might sometimes supplement its action.

Again, autopsies of aphasic patients have not unfrequently revealed lesions of various parts of the encephalon, other than the frontal lobe. But it is easily conceivable that the fibres from this locality, in passing through diseased portions of brain substance, should become affected, even though their centre remained healthy. In this case the cause of the aphasia would be no indication of the seat of the faculty of speech. It is presumable, moreover, that there exist varieties in the lesions, to which the clinical varieties correspond. In permanent aphasia, the lesion generally consists in softening, especially such as results from obliteration of the middle cerebral artery. Such obliteration frequently determines a hemiplegia at the same time, on account of the distribution of the artery to the corpus striatum. In cases of sudden hemiplegia, M. Fabre considers that the coincidence of aphasia alone permits the diagnosis of obliteration instead of hæmorrhage, as the cause of the accident.

Transitory aphasia either depends upon neuroses, as hysteria or epilepsy, or is attributed to congestions. But M. Fabre is inclined to rule out this last circumstance, and substitute obliterations of artereoles, which cause a temporary derangement of the nutrition. After a while the development of collateral circulation renews the nutritive activity of the region, and the patients recover.

No therapeutic indication can at present be based upon this fact of arterial obliteration as the most common cause of aphasia, but it may tend to prevent the trial of useless or untimely measures.

Pulmonary Emboli as a Consequence of Congelation of the Limbs.

At Strasbourg, the opening lecture of the course of operative surgery, delivered by Professor Michel, consisted in an interesting study upon pulmonary emboli as a consequence of congelation of the limbs. At first sight this consequence seems to be extremely far fetched, but the links are easily traced by means of such experiments as those made by M. Powchet on animals. The following are the conclusions of a memoir submitted by him to the Academy:

1st. The first phenomenon produced by the cold is the contraction of the capillary vessels to such an extent that no globule can enter them.

2d. Presently the blood globules begin to alter, become granular, opaque, crumpled on the edges. If only the limbs have been frozen, about the fifteenth or twentieth part of the globules are altered; but if the entire body has submitted to the cold, nearly all the globules are disorganized. In this case, the animal dies inevitably.

3d. When the congelation is partial, the frozen part is destroyed by gangrene. If it be of small extent, the amount of disorganized globules poured into the blood is often not sufficient to compromise life.

4th. If a large extent of surface has been frozen, and then suddenly thawed, so that a quantity of disorganized blood globules are thrown into the circulation, the animal is liable to die on account of this alteration of the blood, and by no means in consequence of stupefaction of the nervous system. Hence it follows that the chances of life are increased in proportion to the moderation with which the thawing-out process is conducted.

M. Michel, supported by the case of a patient at the hospital, who exhibited symptoms of asphyxia after her frozen feet had been thawed, admits that the danger results, not merely from the presence of disorganized globules in the blood, but their presence in the pulmonary capillaries. The accidents occur only after sufficient time has elapsed for the formation of clots from dead globules, then separation from the main mass in the large veins, and their arrival in the lungs. Here are found obstructing the capillaries, long clots, containing little whitish grains which seem evidently to be formed by altered blood globules, fat globules, and

fusiform epithelial cells. The more recent clots surrounding these grains, and the infarctus found in the pulmonary parenchyma, prove an arrest of the current of blood in the lung, on account of their presence in the capillaries. These lesions explain the symptoms observed in such cases, the frequency of the respiration supplementing the impermeable portions of the lung, the presence of râles due to the sero-sanguinolent exudation that succeeds the embarrassment of the circulation, the bluish tint of the face, coinciding with a certain pallor of the skin. Death may result from syncope caused by the simultaneous formation of a great number of the pulmonary emboli in the capillaries, or even, and more suddenly, by the obstruction of the pulmonary aorta itself. Larrey relates a case during the Russian campaign, where this seems to have happened: "The chief pharmaceutist, Zurean, arrived at Kawno, exhausted with hunger and cold, and passed several hours in a warm room. Immediately his frozen limbs became swollen, and he expired without uttering a word."

One of two destinies is reserved for the microscopic clots of blood globules. They either degenerate, and in consequence of their molecular disorganization the capillaries are reopened; or they organize by means of the development of fusiform cells, and then the capillaries are definitely obstructed. The method of elimination from the general circulation is at present unknown.

In either of the foregoing cases, the patient may recover. The danger is aways in proportion to the EXTENT of the lesion, and the SUDDENNESS with which it is produced.

Uterine Retroversion During Pregnancy

Dr. Vignard, of Nantes, communicates to the *Journal de l' Ouest*, two observations of uterine retroversion occurring, one at the third month, the other at the fourth month of pregnancy.

In the first case, a difficulty of micturition existed for several days, and was followed by an attack at night of acute hypogastric colic, accompanied by intense vesical tenesmus. In the morning these symptoms abated, to give place to severe pains in the back and the groins, and particularly, a most painful pressure on the rectum. On examination, a tumor was discovered in the hypogastric region, extending 10–12 centimetres above the symphysis. This was formed by the distended bladder.

By the vaginal touch, an immobile tumor was discovered, extending from the sacral concavity to the pubes, and forming to the vagina a convex roof, perfectly uniform, but slightly inclined downwards and backwards. The neck of the uterus was discovered with difficulty, forcibly pressed against the upper part of the posterior face of the pubes. No fluctuation was perceived between the hand placed on the vesical tumor and the finger pressed against the tumor in the vagina. Pressure on the abdomen did not in the least affect the roof of the vagina.

The bladder and rectum were evacuated by the sound, and an injection, and the physician then attempted the reduction of the uterus. After various methods had been tried in vain, the following proved successful:

The patient was placed on the back, the head tolerably low, the thighs separated widely, the feet on two high chairs, and the pelvis supported on a pillow placed at the edge of the bed. The physician then introduced the four fingers of the right hand, one after another, into the vagina and taking with the left hand a *point d'appui* on the pubes, he forcibly pushed the uterus in a direction directly upwards. The tumor did not budge. Upon this the tactics were changed, and the operator directed his fingers forcibly, but with extreme slowness, toward the sacro-vertebral angle, gliding around the tumor, and keeping the radial border of the hand as near as possible to the pubic arch. It was tolerably easy to arrive at the promontory, and at the same moment the uterus seemed mobilized. Upon withdrawing his hand, Dr. Vignard discovered the neck of the womb returned to the centre of the vagina. Abdominal palpitation discovered the body of the uterus above the pubes, replacing the void that had been left after the evacuation of the bladder. The only indications afforded during the operation that the reduction had been effected, were the slight mobility of the uterus, and the contact of the fingers with the sacro-vertebral angle. Dr. Vignard thinks that the operator may be sure that he has succeeded, every time that the diameter sacro-sus-pubien can be occupied by the radial border of the hand.

The patient was recommended to recline upon the abdomen during the first day, and the uterus retained its normal position. The subsequent pregnancy and the accouchement were unaccompanied by accident, but the child, who had vomited bile in

the amnion, continued to vomit after birth, and died in seventeen hours, with the signs of acute peritonitis.

In the second case, the third degree of retroversion seemed to have been attained, and the vaginal cavity was completely occupied by a globular body, warm, firm, elastic, that seemed to be the posterior face of the uterus. The neck forcibly flexed, was discovered high up behind the pubes. The retention of urine was considerable, but easily relieved by catheterism. The reduction was first attempted by the attending physician, but he found it impossible to raise the uterus above the superior strait. M. Vignard then practised the manœuvre already described. The four fingers were pushed directly backwards in the sacro-pubien diameter of the pelvis, while the radial border of the hand pressed forcibly against the pubic arch. The pressure was as moderate as possible, to avoid injury to the fœtus. At the moment that the fingers touched the upper part of the sacrum, a faint crackling sound was heard, the resistance was felt to be vanquished, and the vagina free. On withdrawing the hand, the neck of the womb was found returned to its place. The patient continued her pregnancy in safety, and was delivered at term of a healthy child.

M. Vignard passes in review several methods that have been proposed for remedying this serious accident of retroversion.

The method of Burns consists in placing the patient on the belly, and keeping the bladder perfectly empty by repeated catheterism. This method can only be successful in the first degree, in which the long axis of the uterus is parallel to the sacro-pubic diameter. This, however, was the case in Vignard's first observation, but the method was tried and failed.

Boyer's direction, to draw down the neck of the womb at the same moment that the body is pushed upwards, is regarded as at least superfluous, since the neck returns of itself when the immobility of the body has been overcome.

Moreau's plan of hooking the index finger around the neck, is condemned as futile.

Negrier introduces the entire hand into the vagina, and pushes the uterus *en masse* as in certain methods for reducing hernias. A very large surface is here attacked at once, the tumor is flattened, and the inferior portion therefore increased in size, so that the method is more painful and more difficult than that of Vignard.

M. Vignard rejects all methods by the rectum, because the hand will find more difficulty in reaching the promontory by this route than by the vagina, and all efforts to push the uterus directly upward tend merely to press it against the promontory which forms an insuperable barrier to its ascent. In resuming his own method, M. Vignard observes, that before directing the fingers toward the sacrum, he pushes upward on the anterior part of the uterus, and then glides toward the body of the organ behind.

Tumors of the Tongue and Pharynx—New Operation

M. Desgranges publishes in the *Journal de Lyon*, certain considerations on tumors of the tongue and pharynx, and a special method for operating upon them. This method belongs to M. Sedillot, and consists of a section of the lower maxilla on the median line, by means of which the two halves of the bone could be drawn aside and sufficient space left to excise the tumor. The wound of the soft parts heals readily, but for the cicatrization of the segments of the maxilla it was found necessary to maintain the adjustment by means of pincers. This instrument presents certain inconveniences, and M. Desgranges has used metallic sutures instead, piercing the bone with a drill, for the passage of the silver wire.

Two cases are related where this operation was successfully performed for an epithelial cancer of the floor of the mouth. In the first case, the tumor, situated under the tongue, extended from the first molar of the left side to the canine at the right. The posterior face of the maxilla was invaded, and the incisors and left canine were partially loosened from the alveoli.

In operating, the integuments were divided as far as the hyoid bone, then the section of the maxilla effected by the chain saw. Care was taken that the section should be made at the left side, and the insertions of the genio-hyoid and genio-glossal muscles of the right side avoided. Upon separating the segments of the bone, the diseased parts were easily removed with curved scissors, without touching the subjacent muscles. No blood fell into the pharynx, so that suffocation was avoided. The results were most happy. The tongue retained its movements, and no trouble occurred in the respiration. The two halves of the maxilla were not displaced, and when the patient left the hospital three weeks after the operation, a fibrous callus united the segments,

and with sufficient solidity to permit movements of the entire jaw.

In the second case, the tumor had burrowed more deeply, and was ulcerated. The superficial layers of muscles were removed, but enough remained to insure the movements of the tongue. The operation, performed exactly as in the preceding case, was followed by a slight attack of erysipelas, and it was a month before the two halves of the divided maxilla ceased to shake in the movements of the lower jaw. But in six weeks the osseous union was complete.

This preliminary osteotomy opens a free route to the bistoury; it enables the operator to examine the entire tumor, and to pursue its prolongations, a circumstance essential as a guarantee against relapse. Moreover, the extreme difficulty of ligating the numerous arteries encountered in this region is greatly palliated, and finally, the danger avoided of suffocation during the anæsthetic sleep, on account of blood flowing into the larynx.

P. C. M.

The Appointment of Hospital Internes in Paris—Interesting Cases from Cliniques of M. Gosselin

To the Editor of the Medical Record.

Sir—I cannot resist the opportunity to say a word on the admirable system that obtains in Paris for the regulation of the hospital studies of the pupils. Admirable both for its democratic equity in throwing open the best clinical advantages to all who choose to try for them, and for the stimulating pressure that it exerts on the mental exertions of the young men. Instead of private cliques surrounding each hospital physician—consisting of his paying students, to whom his only equivalent for three hundred dollars is the prospect of nomination to a vacant place in the wards—there is a perfectly free competition by means of nomination before a jury.

Two sorts of places are directed to be filled by the students in the hospitals. The lowest is that of externe. An externe is obliged to be on hand at every morning visit (a certain number of absences in the course of a year occasions the forfeiture of the place), and with his companions, records the prescriptions, and performs certain personal services required for the patients, as the

dressing of wounds, application of blisters, &c. A definite number of externes is attached to each service, the number varying, of course, with the extent of the service. To secure a place in this body, a medical student inscribes himself for the tria-examinations, which are conducted on two year subjects given at the moment, one pathological, the other anatomical. Two examinations take place at each concours. In the first the candidates prepare written answers to the questions, during a half hour allotted for the purpose. Upon the results of this preliminary examination, a certain number of candidates are estimated, and the selected minority are submitted to a second final examination of the same nature, but which is oral. The number of places to be filled each year, is sufficiently large to give nearly every serious student a chance for the external.

For the place of interne, corresponding to what we call resident physician, the externes alone are eligible candidates. There are about forty-five places, and two hundred and eighty competitors.

The examinations (which occupy two or three months) are of the same character as those of the external, only considerably more difficult. An externe generally calculates to compete twice before he succeeds in becoming interne. To prepare for these competitions, the candidates hold conferences in groups of twenty or thirty, that continue throughout the year previous to the moment of trial, and in which the entire outlines of internal and external pathology are passed in review.

An externe is nominated for three years, an interne for four; and each year is passed at a different hospital. A person may become a competitor for either position at any stage of his medical studies at which he feels himself sufficiently *fort* to stand the examination.

It is plain that the benefits of this system—great as they are to the students who succeed at the examinations—extend also to those who fail, since all are equally compelled to prolonged, thorough, and systematic work. No one can observe the working of the method without wishing for its introduction at home.

Another excellent custom in relation to the management of hospitals, is that of making the visit between eight and nine o'clock in the morning. M. Gosselin, the successor of Velpeau, for instance, is always on hand at eight precisely, and calls

the roll of the students with the exactitude of a drill sergeant. Woe betide him who has overslept himself, and who, in spite of a breakfastless race through the quarter, arrives after the *feuille de présence* has been laid upon the table by the inexorable chief.

"Why is this ulcer not dressed, sir?" demands the surgeon, fixing the trembling externe with his bright black eyes.

"I, I—excuse me, I was late. I meant to do it after the visit."

"Attend to it immediately, and never tell me again that you were late. That is no sort of reason for neglecting your duty. You are not to be late."

All honor to men who, knowing their own duty, know also how to keep others up to the mark. All shame, confusion, and perplexity to those who, careless, indulgent, or shiftless, permit things to be left undone that ought to have been done—after the fashion of all miserable sinners!

As M. Gosselin's clinique lasts three hours, his extreme punctuality alone saves for the student the bulk of the day intact, and able to be employed at lectures, dissections, libraries, etc. After experiencing all the benefits of this system, I feel a certain horror of that prevailing in New York, where the visit is made at any time between twelve and three, subject to all sorts of variations dependent upon the exigencies of the physician's *clientèle* or caprice.

Cases of Metrorrhagia

Two cases of metrorrhagia have formed for M. Gosselin the themes of recent and interesting clinical lectures. In the first case the hæmorrhage had come on after a suppression of menstruation during two months, and the question of spontaneous or provoked abortion immediately suggested itself. M. Gosselin recapitulated the circumstances of the diagnosis which led him to rest finally upon this suggestion. The uterine orifice was neither granulated nor occupied by a polypus. Neither cancerous nor fibrous tumor could be discovered. Ballottement of the uterus was somewhat painful. The body was sufficiently voluminous to be felt in the hypogastrium; the orifice sufficiently open to permit the introduction of the index finger.

Under these circumstances, in spite of the affirmations of the woman that such contingency was impossible (affirmations which, as every practitioner knows, are precisely what create the delicacy

and difficulty of the diagnosis), M. Gosselin did not hesitate to pronounce for an abortion. Principally on account of the denial of the patient, the inference was further drawn, that the abortion was deliberate. The hæmorrhage ceased spontaneously, shortly after admission to the hospital, and the principal danger that remained to fear, was that of a metritis, determined by the instrument that had been employed. Hence, while active treatment was superfluous, active surveillance was imperatively required.

The other case was much more serious, and was first mentioned in connection with the autopsy of its subject.

The patient had arrived in a state of exhaustion, too great to admit of a precise examination, but complaining of an abundant uterine hæmorrhage. Small vegetations were discovered around and within the os uteri, which, though apparently different from the tumefaction of cancer, proved at the autopsy to be carcinomatous. A hard tumor of some size was discovered behind and above the vaginal cul-de-sac. M. Gosselin could not decide satisfactorily to himself whether the body of the uterus was simply inflamed, or the seat of a cancerous tumor.

The day after admission the patient was seized with a most intensely acute peritonitis. The constipation was obstinate, and presently accompanied by vomiting of matters that, though destitute of stercoral odor, resembled the contents of the small intestine. It was not the green liquid usually vomited in peritonitis, nor that tinged with brown occasionally observed, but distinctly brown, and characteristic of intestinal obstruction, especially a strangulated hernia. No trace of hernia, however, could be discovered, and the conclusion was arrived at, that the obstruction was caused by intestinal adherences dependent upon the peritonitis.

At the autopsy, adhesions between the intestines and uterus were found in fact to be sufficiently extensive, and the intestines were so agglomerated around the pelvic cavity, that separation of the organs was attended with considerable difficulty. Behind the uterus was a cavity as large as a man's fist, circumscribed by the loops of intestine, by the uterus, and the abdominal walls, and containing a quantity of fæcal matter, poured out from the intestine by three or four large openings.

Such openings constitute an unusual lesion under the circum-

stances. According to M. Gosselin, the uterine cancroid, which extended from the neck into the body of the womb, had been the point of departure of the whole train of circumstances. The irritation of this tumor had first developed the effusion of plastic lymph between the uterus and the intestines, which united these organs by the firm adhesions noticed above. Extension of this subacute inflammation had gradually thinned the coats of the intestine, until, at a given moment, the internal tunic gave way, and the contents were poured into the pelvis, exciting the acute peritonitis which had carried off the patient. The increased obstruction, upon which depended the stercoraceous vomiting, was evidently, as had been supposed, the result of the rapid formation of lymph during the period of acute inflammation.

Empyema and its Treatment by Perpetual Drainage.

An extremely valuable clinique was that held by M. Gosselin on a case of empyema, that he had had under his eyes for two years, and in relation to which he suggested several ideas that are not everywhere current.

Until recently (observed the Professor) suppuration of the pleura was regarded as a necessarily fatal disease, both on account of the exhaustion induced by the long continued drain on the system, as also by the habitual coexistence of grave pulmonary disease. No cure is possible unless on the condition of entirely evacuating the pleural cavity, which can only be effected spontaneously by the establishment of a bronchial or cutaneous fistula. In a few cases children have been known to recover after the establishment of the first kind of fistula, or vomica, as it is technically called, but only uncertain reliance can be placed upon the benignity of this mode of evacuation, and no physician has the right to provoke it. On the other hand, the cutaneous fistula is even more dangerous, air insinuates itself into the cavity, decomposes the pus, and prevents the dilatation of the lungs, which gradually assume a state of definite collapse. Hectic fever sets in with all its train of symptoms, cough, diarrhœa, and everything indicating the absorption of purulent matters, and the patient is generally carried off in two or three months at the furthest. Modern surgery, however, has ventured to interpose the operation of thoracentesis as an attempt to arrest the fatal march of

this serious disease. This operation, whether performed by simple puncture or by incision, is (according to Gosselin) essentially the same, and essentially useless unless accompanied by a certain precaution presently to be described. In the first case the little wound speedily cicatrizes, and a repetition of tappings, is required, which finally results in the establishment of a fistula. By this the pus indeed escapes, but the air also enters, with the consequences above described. The same thing is true of an incision, and although there was more chance of success after Sedillot suggested counter-openings, and the use of injections to wash out the cavity, the results were still far from satisfactory. M. Chassaignac, however, has had the happy idea of inserting by the two openings perforated caoutchouc drainage tubes, which afford free and continual exit to the pus, and thus neutralize any evil effects resulting from the inevitable ingress of air. For the pus, however decomposed, is innoxious if able to freely escape, instead of being shut up in a close cavity, and stimulating its own absorption.

In addition to the use of drainage tubes, injections of warm water are made every two or three days. The patient who furnished the occasion for these remarks, had been treated by the method above described, which had proved remarkably successful. He had first come under the care of M. Gosselin two years ago at La Pitié, and appeared then in a dying condition, exhausted by a long standing empyema and thoracic fistula. As soon as free exit was afforded to the pus, and the drainage tube established, the hectic fever began to mend, the patient's strength rallied, and in three months the convalescence seemed so solidly established, that the drainage tube was removed, and the man left the hospital. The flow of purulent liquid had entirely ceased. After working for about three months the patient began a second time to suffer from oppression. A fistula reopened, and after some weeks the general health had fallen to nearly as desperate a condition as on the first occasion. Readmitted to La Pitié, and treated again by a drainage tube, the patient again rapidly recovered. After this experience, the tube was left permanently in place. A third time he had run down in strength, and entered La Charité, but was speedily built up again by the same treatment, and thoracic injections of iodine and of sulphite of soda. It was M. Gosselin's intention to leave the drainage tube in place

until the pleural cavity should be entirely obliterated. And this practice, and the theory upon which it is founded, constitutes the original part of his lecture. He declares that it is absurd and chimerical to hope that a serous membrane that has undergone a pyogenic transformation, can ever regain its original character or functions. So long, therefore, as it exists, so long will there be drainage from renewed secretion of pus. But by prolonged care in carrying off the corroding secretion as it forms, the surgeon may hope for the formation of adhesions which shall definitely obliterate the cavity, and constitute the cure of the disease.

By means of these combined methods, therefore, judiciously applied, many patients, in even grave stages of hectic fever, may be snatched from the jaws of death, and restored to a tolerable degree of health. This, of course, cannot be expected if the empyema complicates advanced tuberculous disease.

M. Gosselin also applies the system of perpetual drainage to abscesses situated under the great pectoral, and whose evacuation is rendered difficult by the tonicity of the muscle. A case of this kind, actually in the ward, is doing extremely well. The tube, of course, passes through the original and the counter incision. Injections are made every two days with warm water.

Psoas-Iliac Abscess.

A very different kind of abscess was that presented by a case admitted January 7th. This was a young woman, of rather lymphatic temperament, who had suffered for six months with pain in the sacrum, and, for a month in addition with pain in the left groin. In complete repose the patient was conscious of no suffering, but the least movement awakened the pains, as also pressure in the affected regions. Lying on her back, the patient was unable to completely extend the left thigh, and forced flexion of the limb was painful; when the patient attempted herself to flex the thigh on the abdomen, the lumbar vertebræ arched forward. This same forward projection of the lumbar part of the spine was very evident in walking, when also the patient limped, and rested principally on the right leg. You will recognize this curvature as a symptom of insufficiency of action on the part of the psoas muscle. It would seem to be an instinctive attempt, by bringing the fixed insertions of this principal flexor of the thigh in a direc-

tion approaching a perpendicular to the lesser trochanter, to supplement the intrinsic deficiency of power by the more favorable direction in which it was enabled to act.

Still another sign was obtained by placing the patient on the abdomen, and after seizure of the ankle, bringing alternately the right and left limb into forced extension. The left offered a resistance altogether abnormal. Finally, deep pressure in the groin, just above Poupart's ligament, detected an obscure tumefaction, though not fluctuation.

In forming the diagnosis, M. Gosselin first set aside the possibility of lumbago, which would have tormented the patient even during repose, and been probably accompanied by rheumatism elsewhere; and of uterine disease, indicated by no other symptoms; and arrived at the discussion of some different forms of spinal disease. The pain in the sacrum must in fact be referred to an affection of the spinal cord itself, of its membranes, or of its bony casement. In the first two cases, however, the sensibility or mobility of the limbs could hardly fail to be affected while the patient in question offered no sign of lesion of either. There remained, therefore, only arthritis of the sacral vertebræ, which tended to terminate in suppuration, if that were not already commenced.

On the other hand, unquestionable symptoms (recapitulated above) indicated inflammation in the neighborhood of the psoas muscle. The possibility of simple chronic psoitis, or inflammation of the surrounding cellular tissue, was eliminated on account of its extreme rarity, except as a consequence of puerperal inflammation. There remained, therefore, after combination of all the facts, the conclusion of a psoas-iliac abscess by congestion, resulting from caries of the lumbar or sacral vertebræ.

The prognosis (pursued the Professor) is excessively grave, and contrasts strikingly with the apparent benignity of the disease at the present moment. Sooner or later, the abscess will probably open, and the patient succumb to exhaustion from the discharge. The only chance is that derived from the use of tonics, and the local application of iodine with the faint hope that the contents of the abscess may be absorbed. Even in that case the spinal disease would continually tend to occasion the formation of another.

Danger of Apparatus in some Cases of Fracture of Jaw.

Three different cases of fracture, one of the lower maxilla, one of the fibula, and one of the radius, furnished the occasion for some pointed and suggestive remarks. The first case was the result of a kick received on the jaw, and the fracture, though distinctly indicated by crepitation, was accompanied by but slight displacement, and an insignificant wound of the mucous membrane. The accident was therefore slight in reality, but Gosselin pointed out a certain possibility of grave danger from an unlooked-for source. He declared that whenever, as in this case, a solution of continuity had been effected inside the mouth, the application of any apparatus for holding in place the fragments of the jaw-bone was extremely mischievous. In two cases observed at La Pitié, irritation of these machines induced extensive inflammation of the mucous and submucous tissue, ultimately reaching the bone, and exciting osteitis, followed by denudation, necrosis, purulent absorption and infection, and death. Even where death is not the result, the necrosis compels an elimination which often lasts four or five months.

In view of these possible perils (upon which, says Gosselin, authors have not sufficiently insisted), all apparatus should be proscribed, and the fragments retained in place by a simple bandage. The slight deformity resulting from lack of perfectly accurate adjustment, is more than compensated by the security for the life of the patient.

Treatment of Fracture of the Radius, etc.

Similarly, was an unexpected complication indicated as the occasional result of an accident so simple and seemingly harmless as fracture of the radius. The danger is again due to want of care in the application of the apparatus. A patient comes to the consultations the first day of the accident to have the bone "set" and arm splints adjusted, and insists on returning home, where he will be withdrawn from surveillance. The second or third day the arm swells and becomes so intensely tightened by the splints that, if they be not removed, the inflammation may result in gangrene. Gosselin had seen some examples of this consequence.

In the case in question, when the patient returned to the hospital, after suffering for two days with sharp lancinating pains

and sense of constriction in the arm, the limb was found greatly swollen, and a bright red streak on the back indicated the approach of gangrene. This was happily warded off by the removal of the splints, but the danger had been imminent.

It is, therefore, a rule with M. Gosselin, in all cases of fracture of the arm, especially with female patients, children, or old people, to wait during four or five days after the accident for the application of the splints. During this time the part is kept constantly poulticed, and at the end the inflammation is found to be well reduced, and no difficulty opposes itself to the setting and adjustment of the fragments, as the callus has still hardly begun to be formed.

For further precaution, such an apparatus is selected as shall leave the limb open to observation. Tampons of cotton, wool, and then stout rolled compresses are placed against the free ends of bone to press them into place; a splint is adjusted to the posterior and anterior face of the arm, and retained by two or three bands of diachylon.

In the case of fracture of the fibula and external malleolus, the leg was placed, after adjustments of the fragments, in a simple plaster casing, formed of bands of tarletan, dipped in liquid plaster. This was chosen because it alone becomes fixed in a few minutes, while dextrined, gelatinized, or silicated bands require some time to attain the necessary rigidity. Where, therefore, the fracture only involves a small bone, as in this case, and there is less dread from the possible breaking of the case, M. Gosselin thinks that plaster is preferable to all other material for immobility.

Dangers of Erosions of Urinary Passages in Cases of Retention —Reabsorption of the Retained Urine—Uræmia.

The case of a man who died shortly after admission to the hospital for a retention of urine caused by urethral stricture, furnished opportunity for an acute suggestion from the lecturer. During life, the sinister progress of the disease had been suspected to depend upon concealed inflammation of the kidneys, possibly an abscess, whose presence was betrayed only by the purulent infarction which proved fatal. But at the autopsy, the kidneys were found to be perfectly healthy, and some small erosions of the urethra and the bladder were the only lesions discovered to explain the death.

These lesions, however, were not sufficiently extensive to have caused death directly, but their indirect agency might be explained in one of two ways. It might be supposed that one or both kidneys had ceased to secrete, their functions being interrupted by sympathy with the interruption of the excretion. In this case, death would result from intoxication, caused by accumulation of urea in the blood. But the patient had exhibited no signs of coma, nor the fever characteristic of ordinary uræmic intoxication. M. Gosselin inclined to adopt the other theory, which suggested *reabsorption of the retained urine*, at the eroded surfaces of the urethra and bladder. A special uræmia would therefore result, betrayed by somewhat anomalous symptoms, but leading to definitely fatal results as that dependent upon suppression of urine.

M. Gosselin explained that his reason for insisting upon this mode of fatal termination, was to point out the danger of even small erosions of the urinary passages, in cases of retention of urine from any cause. With the possibility of this danger in mind, the surgeon would often be much more careful than at present, to avoid tearing the mucous membrane by any instrument employed in treatment.

Diagnosis of Cancerous Stricture of Rectum.

Nearly at the same time, a patient died with stricture of the rectum, and in exposing the result of the autopsy, M. Gosselin recapitulated the clinical details of the case. The patient had entered the wards only eight days previous to his death, and at that time the stricture was so narrow that the little finger of the surgeon could hardly penetrate into the rectum across it. Below the stricture the finger perceived a rough mammillated surface, and the tissues around the narrowing were extremely hard and resistant. These circumstances, joined to the profound emaciation and exhaustion of the patient, excited some suspicion of cancer. But it is rare that cancer is equally disseminated over all the surface of the rectum, or produces a stricture so narrow or so near the sphincter. Cancerous stricture is never impassable to the finger, as was the lesion in question. Moreover, a cancer would not have remained rigid so long a time, but ulcerated considerably before the five years that had elapsed since the beginning of this one.

Cancer being eliminated, the diagnosis turned upon fibrous thickening, probably of course under the influence of syphilis.

An operation was decided upon, and two or three incisions were made at the level of the stricture. Before the operation, however, the patient had been attacked with a chill and some fever, both of which returned with renewed intensity afterwards, and death occurred in consequence of purulent infarction. A metastatic abscess was found in the lungs.

Locally, the autopsy revealed an abrupt stricture, caused by hypertrophy of fibrous tissue, and accompanied above its upper border by a large shallow ulceration of the mucous membrane of the rectum. A certain amount of pus covered the surface denuded of epithelium. This ulcer, said the Professor, added greatly to the gravity of the disease, determining the tenesmic diarrhœa which had exhausted the patient, and probably constituted the immediate cause of death.

P. C. M.

The Catalogue of the U. S. Army Medical Museum, and a Foreigner's Estimate of the Medical Resources of America—Two Interesting Cases of Ovariotomy—The Use of Drainage Tubes—Guerin's Pneumatic Occlusion.

PARIS, February 9, 1868.

To the Editor of the Medical Record.

SIR—The Archives of Medicine, in a very complimentary note, acknowledges the gift of the catalogue of the United States Army Medical Museum.

Every one interested in the advancement of medicine and the amelioration of the health of armies, ought to feel indebted to the American Government for the gigantic and costly enterprise which it has just undertaken. The magnificent volume which has been sent us by the liberality of the Surgeon-General cannot fail to be of immense utility, even to us to whom the rich Museum is inaccessible. Thanks to the summary observations which it contains of each piece registered in the Museum, we are able, in a great measure, to study the collection as if it were under our eyes, and shall in the future have the means of controlling the quotations of American surgeons who shall take their illustrations from the Museum.

Our administration is not accustomed to such generous initiative in the distribution of official reports to the journals. It has been necessary for the American Government to have the honor of giving the example, and of meeting, with the most laudable munificence, the silent wishes of the medical press.

A Foreigner's Estimate of the Medical Resources of America.

I met, the other day, a young Norwegian surgeon, with hair as yellow and eyes as blue as became a countryman of the Viking, who had been spending some months in the study of this Museum, and expressed for it the most unqualified admiration. "I cannot understand," he said, "why you Americans should take the trouble to come to Paris to study surgery; your facilities at home are worth fully as much, if not more, than all you can get here."

Cases of Ovariotomy.

Certainly no one need come to Paris to study ovariotomy. M. Richet, than whom a more distinguished surgeon is hardly living, has just had the misfortune to add another to the list of failures in this formidable, but sometimes successful operation. He had undertaken the extirpation of the cyst, to comply with the urgency of a brother physician, who considered the case remarkably favorable. M. Richet, however, pronounced an unfavorable prognosis, on account of a circumstance, which he has been subsequently led to consider sufficiently characteristic to serve as a formal contra-indication to an operation. The abdomen of the patient was the seat of a fluctuating tumor, apparently a unilocular cyst; but it had not the form which should have been given by a cyst of so considerable size. Instead of being prominent towards the middle, and advancing as it were in a point, the belly was rather flattened, and much enlarged at the sides but not at all prominent in the middle. In the meantime the surface was even, and no sign existed of a division of the cyst into cavities; the unequal juxtaposition of whose walls might explain the flattening of the abdomen.

M. Richet could not well account for this circumstance, but it inspired him with an indefinable apprehension of evil consequences, an apprehension only too well realized. For when, after incision through the integuments and subperitoneal tissue, both of which were thickened by adipose tissue and much infiltrated, the surgeon arrived in the cavity of the peritoneum, the most solid adhesions were discovered, uniting the cyst to the abdominal walls. The first could be turned with the hand, but they presently became so solid as to resist all efforts. Convinced that localized adhesive peritonitis and fibrinous adhesions were more

readily formed in the pelvic cavity than towards the abdominal walls, M. Richet inferred that the obstacles met with in this latter locality would be re-encountered, and on even a more formidable scale, towards the base of the tumor. He therefore resolved to abandon the operation, and the incision was united with a few metallic sutures.

The patient, however, died of peritonitis the next evening, and the autopsy fully confirmed the prevision of M. Richet. The cyst could only be separated from the abdominal wall by tearing a part of this latter; and in the pelvic cavity the adhesions were so close, that a slow and careful dissection was required to remove them. The bladder and uterus were involved with the tumor. This had no pedicle, properly speaking; it was composed of a principal cavity, from whose wall were suspended, floating, several smaller cysts; it was nourished by means of its intimate and extensive adhesions, especially with the uterus. It is certain that the continuance of the operation would have been completely impossible, since the isolation of the cyst was so difficult, even on the cadaver.

The remarkable flattening of the abdomen was therefore accounted for by the very solid adhesions which maintained it solidly fixed, and drawn downwards. In such cases, concludes M. Richet, ovariotomy should never be attempted.

It is interesting to notice also, that in spite of the repeated attacks of peritonitis which must have occurred to produce the adhesions, the patient had never suffered any abdominal pain, a fact which had greatly conduced to excite the false hopes for the success of the operation.

From Strasbourg, however, comes a note of better cheer. M. Koeberlé has succeeded in saving a patient operated upon for an ovarian cyst, and that in spite of the most formidable complications.

The patient was 43 years old, the mother of three children, and endowed with a vigorous constitution. She was affected with a multilocular cyst of the right ovary, of which one of the subdivisions had ruptured eight months previous to the operation, and occasioned a grave peritonitis. From that time had set in ascites, emaciation, anæmia, and hectic fever. Towards the end of September tapping was performed, and about six litres of brownish liquid, partly serous and partly stringy, were with-

drawn. After this the general health of the patient was notably ameliorated. Ovariotomy was practised on the 26th of November, under the influence of chloroform. An incision was made, twenty-five centimetres in length, giving issue to three litres of reddish serum. Puncture successively of three divisions of the cyst, of which one furnished a yellowish, one a brown, and one a grayish liquid, altogether amounting to eight litres. There remained a multilocular mass, weighing two kilogrammes, which was easily removed after division of a few adhesions, which united it to the omentum and abdominal wall. The former adhesion, which contained large vessels, was destroyed with the actual cautery. The pedicle of the tumor, four centimetres long, was divided by a wire loop, by means of a slip knot. The abdominal cavity was well sponged out, and the incision united by means of four deep, and six superficial sutures. *A glass tube, ten centimetres long, plunging in the pelvic cavity along the posterior wall of the uterus, was placed in the lower angle of the wound, to admit of a free escape of the liquids.* The operation lasted three quarters of an hour. About 400 grammes of blood were lost.

A pelvi-peritonitis occurred, which remained localized and disappeared rapidly under the influence of the free escape afforded to the liquids, and the *half sitting position* given to the patient. But the fourth and fifth day the patient became restless, and the pulse counted 130. On the sixth day the sleep was interrupted at two in the morning, the restlessness augmented to agitation and anxiety; the pulse, still at 130, became variable small, and irregular; inspirations thirty-six; sweats, coated tongue, diminution of the urine, and tympanitis of the abdomen, all announced grave change for the worse.

By the 7th the condition of the patient was extremely menacing. At five o'clock in the afternoon, the surgeon discovered dulness in the right flank, between the iliac crest and the hypochondrium, extending over a space about as large as the palm of the hand. There was evidently a collection of serum, formed during the last fifteen hours, and dependent upon a local peritonitis (probably connected with inflammation of the ovarian vessels), and which would not delay to become general. Bold measures were necessary, and on the spot, M. Koeberlé made an incision in the centre of the dulness, about seven centimetres above the iliac crest. The patient was too feeble to be chloro-

formized, so recourse was had to a local apparatus for the vaporization of ether, which sensibly diminished both the pain and the hæmorrhage. After division of the tissues to the depth of six to seven centimetres, the peritoneum was discovered, and *being opened*, gave issue to about 150 grammes of reddish serum. This was completely withdrawn by means of a canula, the exterior wound united by a single suture, and a tress of lint, replaced subsequently by a glass tube, served to maintain *external communication with the cavity of the peritoneum.* The local dulness had disappeared. The patient was placed in a half-sitting position and in a lateral decubitus towards the right side, in order to facilitate the escape of the liquids.

The next day, the patient, who had been in a subcomatose condition exhibited a marked improvement. The pulse was between 118 and 125, and the respiration twenty-two. On the fourth day after the incision, the borders of the wound were invaded by an erysipelas, which extended about twelve centimetres. Treated with tincture of iodine on the limits of the inflamed parts, the erysipelas was arrested on its third day.

The third crisis attended or consisted in the evacuation by the rectum of gray purulent stools. The patient afterwards became more comfortable, but the tumefaction in the right flank reappeared, and continued, and the surgeon was unable to reach it by sounds introduced, into the wound. Finally, on the eighteenth day after the original operation, the purulent collection opened spontaneously by means of the large tube which had been left in the wound; and a great quantity of pus escaped, and the flow continued during two or three days. The tumefaction diminished in proportion, and disappeared entirely. The tube was gradually shortened, and at the end of a fortnight the cicatrization of the iliac wound was complete, as well as that on the median line, made for the extirpation of the cysts, and where a tube had constantly remained. In a month and a half after the operation, the health of the patient was perfect.

The striking peculiarities of this remarkable case unquestionably belong to the successful plan of leaving the drainage tubes in communication with the peritoneum; and to the boldness which risked an incision of that membrane, to give issue to the products of a local peritonitis. *General* peritonitis was thus warded off, three distinct times—first, in connection with the original oper-

ation, then at the moment of the subsequent tumefaction, and finally during the formation of the abscess. M. Koeberlé remarks, that when in the course of peritonitis a collection of liquids has been formed, opening of the peritoneal cavity is far from presenting the same gravity as when the membrane is healthy. The pseudo-membranes which agglutinate together the intestines, have rendered possible the formation of circumscribed cavities in which the exuded liquids have been able to accumulate, and these serous or purulent foci may be opened, without interesting the remainder of the peritoneal cavity. When these liquids, which have a great tendency to decompose and become fetid, have been evacuated, there is nothing to prevent washing out the cavity with injections of sulphate of soda or of phenic acid. By this means, the affection is reduced to a simple local peritonitis.

M. Koeberlé has performed NINE ovariotomies during the last six months, and only lost one patient, and she was fifty years old, and had submitted eight times to paracentesis.

The Benefits of the Drainage Tubes.

The immense advantage to be derived from the practise of leaving a tube inserted in a cyst, to provide for the complete evacuation of its contents, is shown in a remarkable case of hydatid of the liver, cited by the Archives from an observation of Dr. John Harley.

When the patient first consulted the physician, he was affected with an abdominal tumor of four years' duration, continuous with the liver in the hypochondrium, and extending to within two fingers' breadth of the pubes and Poupart's ligament. Dulness extended from this point to the level of the right nipple. On percussion, fluctuation was evident in all parts of the tumor.

Three times in the course of the first eighteen months of the development of the tumor, the patient had suffered attacks of sharp pain in the abdomen and epigastrium, of which the first attack had lasted twenty-four hours, and the last fifteen days. He had never been jaundiced.

From the seat of the tumor and its development, Dr. Harley diagnosticated an hydatid cyst of the liver. No treatment, was instituted. Two years later the patient returned, with the tumor

somewhat increased in size. The girth measured forty-two and five-eighths inches, and under the influence of a slight attack of local peritonitis, the cyst increased so rapidly, that in ten days the measure of the girth had increased to forty-four and a half inches. In view of this rapid development, it was decided to tap the cyst, which was done on the level of a line going from the xiphoid cartilage to the umbilicus. A clear colorless liquid escaped, whose complete evacuation occupied two hours. Eleven litres of this liquid were collected, and found to contain several broken cysts, the size of a filbert, and cysts unbroken, as large as a pea. The operation was well supported, and relieved the patient. The abdomen retracted, and by palpation, below the umbilicus, could be perceived the lower border of the cyst. The canula was left in place.

The patient remained without fever till the eighth day, when the canula escaped from the wound, and all flow of liquid ceased during twelve hours. The cyst became distended and perceptible in both hypochondria, the skin hot, pulse 120. The canula was replaced, and immediately there escaped 250 grammes of a turbid liquid, dark yellow in color, and with a fetid odor. The febrile symptoms disappeared, while the flow was only interrupted by the fragments of hydatid cysts that from time to time blocked up the canula. When the obstruction became definite, the cyst was distended a second time, and grew painful, and the fever returned. On this occasion, an elastic sound, nine inches long was introduced into the cavity by the canula, and 600 grammes of liquid were collected. About the same amount escaped during the course of the following fortnight, and occasionally, owing apparently to the rupture of some secondary cyst, the flow would become more abundant.

The forty-third day the canula was entirely removed, but the elastic sound left in place. Up to this date injections had been made of water mixed with iodine or creasote, forty drops to a litre. On the fifty-first day, a considerable hæmorrhage was produced in the cyst. The pulse immediately mounted from 96 to 140, and in the evening was 160. The skin became hot, dry, and yellowish, the cyst hard and distending the epigastrium and hypochondria, and the patient vomited repeatedly. 500 grammes of thick fetid sanguinolent liquid, resembling the blood which flows after the section of the liver, were withdrawn from the cyst,

which was then carefully washed out with water, containing some creasote. During the following week, the iodine injection was replaced by a solution of twenty-five to fifty centigrammes of nitrate of silver, in some ounces of water; afterwards an injection was made every morning and evening of a solution of four grammes of sulphate of zinc in 300 grammes of creasotized water.

After several days, during which the stools were quite colorless, there was suddenly evacuated by the rectum a quantity of pultaceous matter, of a color analogous to that of the liquid coming from the cyst. A few days later, a great quantity of pure bile flowed from the wound, fifteen grammes being collected in some minutes. Communication was therefore evidently established, on the one hand with the intestine, on the other with the gall bladder. This was the fifty-third day.

After various less important vicissitudes, it is noticed on the 123d day, that no more bile escaped from the wound, that the cyst was greatly diminished in size, so that the sound, which had penetrated 9 and 10 inches, now extended only 4. On the 148th day, the flow had ceased, and the sound was withdrawn. Shortly afterward, the health of the patient being entirely re-established, he resumed his ordinary occupations. The girth had diminished 13 inches. The dulness of the liver was *normal*, but the spleen remained hypertrophied. The heart had resumed its proper position. No trace remained of the tumor.

Dr. Harley follows the recital of this interesting case with some general remarks on the treatment of hydatid cysts, in which he particularly insists on the necessity for favoring the complete evacuation of the cavity. He thinks that nearly all failures are due to neglect of this precaution and of any attempt to obliterate the hydatic membranes. If any liquid be left, it is sure to putrefy sooner or later, and infect the blood. Then follows a synoptical table of about 100 cases gathered from different authors. In thirty-four, a single opening had been made, followed by complete or partial evacuations of the liquid and immediate closure of the wound. There were eleven cures, thirteen ameliorations, and ten deaths.

In the second table are thirteen cases treated by successive openings, with or without iodine injections; eight ameliorations, two cases without result, and three deaths.

In the third table, containing thirty cases treated by one or several openings followed by prolonged communication with the exterior, there are twenty-three cures, of which at least eighteen may be considered radical, and only seven deaths, five of which must be attributed to a new accumulation of liquids which had been unable to escape, and had putrefied. In ten cases in which the tumor was opened by caustic potassa, were observed three cures, three ameliorations, and four deaths. Dr. Harley thinks, moreover, that the caustic presents no real advantage, and has the disadvantage of being much more painful than the other treatment.

These results therefore tend to confirm the views expressed in connection with the operation for ovarian cysts, namely that the dangers do not depend upon the admission of air into, but the imprisonment of liquids within the cavities, natural or artificial. Escape, escape, escape for all these vile and noxious fluids—such is the watchword of a host of modern surgeons, in a host of cases, and the doctrine is perhaps best applied by M. Maisonneuve, in his apparatus that fulfils at once the double purpose of occlusion of the wound, and aspiration of the liquids at its surface or burrowing in its recesses. I believe I have already described to you this apparatus, or at all events it is well known to you, if only for the reason that every American physician who comes to Paris goes straight to the Hotel Dieu, to see it in operation.

M. Gosselin, at La Charité, carries out the principle of free drainage for other purposes than that of preventing purulent infection. In case of cold and burrowing abscesses, with or without fistulas, he generally inserts a small perforated drainage tube by the original opening, at the same time exercising steady pressure upon the dilated walls of the cavity. In this way he has recently treated with marked success a case of indolent abscess burrowing under the great pectoral muscle, and has now under treatment an abscess at the malleolus, and another resulting from axillary adenitis; a case of rather diffused phlegmon of the neck was similarly treated, but succumbed to the erysipelas which had been imminent from the first day of the disease, much more before the insertion of the drainage tube. This instrument does not in any case seem to provoke superficial irritation around the wound, and what deep-seated irritation may be excited by the pressure of even such a mild foreign body as gutta-percha,

does not seem to pass beyond what is advantageous for stimulating the reparative powers of the secreting surfaces.

Guerin's System of Pneumatic Occlusion.

In a recent séance at the Academy of Sciences, M. Guerin gave a résumé of the applications hitherto made of his system of pneumatic occlusion,—essentially the same as that of Maisonneuve to which I have just alluded. He ranks these applications under four categories.

1st. Wounds and simple surgical operations such as incisions, ablations of cicatrices or of subcutaneous tumors, extractions of foreign bodies from articulations.

2d. Grave operations, such as amputations of limbs, and accidental wounds of the same importance.

3d. Contused wounds, openings of the skin, and simple complicated fractures, that is with perforation of the skin, while the bones are simply broken.

4th. Wounds from fire-arms with dilacerations and destruction of the tissues, fractures with crushing of the bones, and wounds uniting the gravest complications of traumatic lesions.

In the most favorable condition, the pneumatic occlusion produces cicatrization without traumatic fever, and without suppurative inflammation; that is to say, it realizes union by first intention.

In less favorable cases, and when the wound has already been a long time exposed, or contains foreign bodies, or, finally, is complicated with anterior morbid conditions, pneumatic occlusion cannot prevent a certain degree of suppurative inflammation; but in virtue of the continuous aspiration which it exercises, it opposes all accident resulting from the putrefaction and absorption of altered fluids, and in all cases favors, and renders much more rapid, the cicatrization, and consecutive organization of wounds.

P. C. M.

The Treatment of Abscess of the Liver by External Incision—Swallowing of a Fork—Perforation of the Stomach and Colon; Escape of Instrument through an Abscess in the Abdominal Walls—Treatment of Morbus Coxarius from a French point of view.

Difficulties in Childbirth with an Abnormal Pelvis; Interesting Suggestion relative thereto—Cases of Complicated Rheumatism —Nux Vomica in the Dyspepsia of Hypochondriacs—Sulphide of Carbon as a Local Anæsthetic—Dextrine in Varicose Eczema.

To the Editor of the Medical Record.

Sir—The medical experience of any particular country on the diseases peculiar to the locality, serves somewhat as Professor Tyndal's lecture apparatus, which projects upon a screen the magnified representation of operations too delicate to be otherwise perceived by the audience; for the characteristics of maladies that may be inadequately appreciated when observed only at rare intervals, become salient and striking when a number of similar cases are massed together. Hence it is in the study of such masses of facts, that the practitioner becomes able to cope in his own climate with the exceptional cases of disease, for which, however infrequently, he is bound to be prepared.

Abcess of the Liver, etc.

Abscess of the liver, as every one knows, is as common in warm countries as it is rare in our temperate zone. The Medico-Surgical Society of Alexandria (Egypt), has just published the conclusions of a most interesting discussion on the treatment of this formidable degree of hepatic inflammation—conclusions that it cannot be uninteresting to relate to you.

The turning point in the debate, was the question of the utility of puncturing the abscess, and a great number of cases were reported by different members of the Society, in which the effects of the operation could be compared with the march of the disease when treated less energetically. I give you the sum-total of the results, without entering into the details.

The cases may be divided into two groups, the first comprising the abscesses not operated upon, the second, those upon whom the operation was performed. Each group is again subdivided into abscesses the size of a man's fist, called large, and all below this dimension, classed as small.

The first group, abscesses not operated, contains 81 cases, among which there were 58 deaths, 14 cures, and 9 doubtful cases. The mortality was, therefore, 80.55 per cent., the recovery 19.45.

In the second group are 42 cases, of which were 21 deaths and 21 recoveries—mortality 50 per cent., recovery the same. The first subdivision of the first class, in which the abscesses were as large as a fist, or larger, contained 24 cases, with 21 deaths, and 3 recoveries—mortality 87.50 per cent., recovery 12.50.

The second subdivision of this group (abscesses smaller than a fist) comprised 13 cases, of which 9 died, and 4 recovered—mortality 69.23 per cent., recovery 30.76.

In the first subdivision of the second group (large operated abscesses), are 22 cases, 15 deaths, and 7 cures—mortality 68.18 per cent., cures 31.81.

In the second sub-class (small abscesses), are 10 cases, of which 3 died, and 7 recovered—mortality 30 per cent., cure 70 per cent.

It is noticeable that each group contains a number of cases in which the size of the abscess had not been determined with sufficient precision to rank it in either of the sub-classes. In view of these statistics, it was resolved by the Society that, 1st, in *all* cases of hepatic abscess, large or small, the chances for recovery are considerably greater if an operation be performed; 2d, that in cases of small abscesses the operation is *so* favorable that more than two-thirds of the patients are cured.

Among the 14 cases unoperated upon, in which the patients recovered, in 11 the abscess opened spontaneously into the lungs. In two cases, the communication was effected with the intestine, and in one, with the stomach. But generally, whenever the abscess opened anywhere than into the lungs, the rupture proved fatal. This was the case 14 times, where the rupture occurred three times into the peritoneum, four times into the intestine, four times the pleura, once the stomach, once the pericardium, and once the locality is not specified.

Death in all cases, whether following an operation, or occurring by the natural progress of the disease, was determined either by general hectic fever, or by uncontrollable diarrhœa. The latter was the most frequent cause of death after an unsuccessful operation, and generally occurred when the puncture had been delayed to an advanced period of the disease. It was indeed decided by the Society that the operation should be performed as soon as possible after recognition of the abscess, and an exploration made, even when the liver was scarcely painful and no

fluctuation could be distinctly perceived. In default of the most salient symptoms, an experienced observer would almost always pronounce upon the existence of an abscess by the earthy tint of the complexion, accompanied by augmentation of the size of the liver; an extremely obstinate diarrhœa, yielding to no treatment; nocturnal sweats; often periodical fever, chills, and loss of appetite. It is affirmed that the introduction of the exploring trocar, even if the liver be healthy, is not followed by any serious accident.

It was generally agreed that the use of caustic was to be proscribed, as being slow, extremely painful, and possessing no advantages over the bistoury. For the adhesive inflammation desired by the employment of the caustic, is invariably set up around the drainage tube, within 24 hours after puncture by the bistoury.

It was asserted, moreover, that the action of the caustic is not well circumscribed, but is apt to occasion badly suppurating wounds.

The persistence of the drainage tube is a most important element of the treatment. This tube is liable from time to time to become blocked up, in which case it may be withdrawn, cleaned out, and replaced.

Swallowing of a Fork, perforation of Stomach and escape through abdominal walls.

A most remarkable case of traumatic abscess is reported in the *Medical Gazette* of Strasburg, as occurring in an insane asylum at Zutphen. The patient was a woman 64 years old, affected with lypemania, who had swallowed a silver fork for the purpose of committing suicide. She was received into the asylum two days after accomplishing this feat, and the physician had no difficulty in detecting the foreign body in the stomach. The teeth of the fork were in the cardiac portion, directed upwards and forwards, the handle lying backwards, in the pyloric extremity. The patient complained of no pain, only a sensation of weight and oppression at the stomach. During the first days, she was submitted to entire repose, severe diet, and expectation. A slight febrile reaction gradually established itself, and the patient at last complained of pain in the left epigastric region. These symptoms continued without aggravation during three months,

and then gradually subsided. At this time the teeth of the fork disappeared from the place where for so long they had been plainly perceptible, and instead was discovered a singular tumor in the abdomen, to the left of the umbilicus, which occasionally had the air of a gravid uterus at four months. It was impossible to decide upon the nature of the contents of this tumor, in which no sign of the fork could be perceived. The pain was trifling, the pulse at 72; stools easily obtained by enemata. A slight febrile reaction occurred later, but the digestion always remained undisturbed.

Five months later, the tumor, which till then had been quite round, began to point. The abdominal walls were not adherent. In the course of the following month an abscess formed; the integuments gradually reddened and thinned, and the tumor opened spontaneously, and gave issue, first, to a small quantity of pus, then to liquid fæcal matters. About a week later, at the morning visit, the physician was surprised at perceiving the four teeth of a fork behind the abdominal wall, close by the fistulous opening. By prudent manipulation, it became evident that the foreign body was only retained in place by the integuments, and in effect, after a couple of lateral incisions, the fork was easily extracted in the perpendicular direction that it occupied to the abdominal wall. The handle was entirely surrounded by extremely fetid fæcal matters; a great number of crystals of phosphate of lime covered the teeth of the fork, which had turned black from a coating of sulphate of silver.

The patient, who during the last days had suffered a good deal of pain, was immediately relieved after extraction of the fork. The fistula was simply dressed, and healed without difficulty, a firm cicatrix being established by the end of a month. For some time longer, the neighboring parts remained infiltrated, but even this infiltration gradually disappeared, and the patient was completely restored to health.

As the tumor had always remained on the left side of the abdomen, it seemed evident that the fork had not traversed the length of the intestinal tube, but passed directly from the stomach into the transverse colon, after an adhesive inflammation had established solid connection between the two organs. It was inferred that the crystals of lime salt had been deposited on the

teeth which had arrived in the colon, while the handle still remained in the stomach.

It is extremely remarkable that the general health was so slightly deranged by the ten months' sojourn and peregrinations of a foreign body in the stomach and intestines. Perhaps the mental alienation of the patient may be presumed to have blunted the general physical sensibilities, a circumstance frequently observed in the pathology of the insane.

Treatment of Coxalgia from a French point of view.

M. Philipeaux, who has for some time made a sort of specialty of coxalgia and its treatment, has recently published a memoir upon resection of the head of the femur, in cases of this disease that have resisted general treatment, and are conducting their victims to the grave. 96 instances of this operation have been published, since it was first practised by Antony White, of London, in 1821; and half the operations have resulted in radical cures. Surgeons have objected to this operation on the ground that it was unnecessary, since all curable coxalgias could be cured by general treatment; that it was fruitless, since the cotyloid cavity was always affected, as well as the head of the femur; and that any attempt to operate upon this cavity was too dangerous, on account of its proximity to the pelvis. M. Philipeaux admits the seriousness of all these objections, but, in reply to the first, observes that the operation is only proposed as a last resource, in cases where all others have failed; in answer to the second he declares that the lesion of the cotyloid cavity has many more chances to heal, if relieved of the irritation caused by the presence of the diseased femur; and finally, although the danger of applications to a point so near the pelvic cavity is not to be dissimulated, yet the surgeon may in many cases be justified in cauterizing, with circumspection, the acetabulum with the actual cautery, and in all cases may remove the fungosities therein developed. The resection is contra-indicated when pulmonary phthisis, scrofula in the third degree, heart disease, or vertebral caries, complicates the coxalgia. Too great an extent of the local caries is also a contra-indication. The operation is favorable in proportion to the youth of the patient; and the following table of 67 cases shows clearly how success varies with age:

Cases	Age	Cures	Death
19	5 to 9 years	12	7
30	10 to 19 "	20	10
10	20 to 29 "	7	3
5	30 to 39 "	2	3
2	40 to 49 "	2	0
1	50 "	0	1

Spontaneous luxation of the head of the femur is one of the most favorable conditions for resection, but is not, as was at first supposed, indispensable. In 32 cases operated, where this luxation did not exist, are counted 16 cures, 9 deaths, and 7 doubtful cases.

The operation comprehends three periods:

A. First Period. The patient is placed upon the sound side, with the trunk slightly raised, and the lower limbs extended. Anæsthesia, of course, is induced.

The surgeon, standing at the right of the patient, feels for the upper border of the great trochanter, and by his incision describes a semi-lunar flap, whose convexity is inferior. All parts burrowed by fistulas should be included in the incision, and all parts removed which seem incapable of assisting in the cicatrization. The insertion of the trochanter muscles is divided, and the border of the cotyloid cavity attained. When all the articulation is carious, the capsule is swollen and often perforated. If it be yet intact, the limb is placed in flexion and abduction before dividing the capsule by a pointed bistoury. The membrane is then loosened above and below by means of a bistoury guarded by a button. In the majority of cases, the round ligament no longer exists; when it does, it is to be cut with this same instrument.

B. Second Period. Luxation.—Forced luxation should always precede section of the femur, except where the parts are united by osseous stalactites. When the femur is intact it is easy to use it as a lever, and execute with it movements of adduction and inward rotation, which rapidly drive the head out of the cotyloid cavity and the lips of the wound.

C. Third Period. Resection.—A small board is then placed behind the dislocated head, the neck denuded of its periosteum (of which as much as possible should be preserved), and then severed by means of a straight or chain saw. If, on examination of the surface of section, any diseased bone is found to have

been left, it is removed by a second stroke of the saw, which sometimes goes below the small trochanter. The great trochanter should be removed in any case, says M. Philipeaux, following Malgaigne, for if left, it will fit itself into the cotyloid cavity, and so oppose the free issue of pus. Finally, all articular fungosities should be removed, and if necessary, the acetabulum rasped, gouged, or cauterized.

D. Consecutive Treatment.—After the operation is terminated the patient is placed in dorsal decubitus, and the sound side of the body somewhat elevated by means of oat cushions, so as to favor the flow of liquids from the wound.

Mattressed gutters for the reception of the operated limb are rejected as useless, fatiguing, and greatly interfering with the dressing of the hip. M. Philipeaux prefers to simply support the patient by the cushions. In two classes of cases, however, it is necessary to maintain continued extension of the leg: 1st, when the surgeon has been unable to place the femur in complete extension during the anæsthetic sleep. 2d, when, after a spontaneous luxation, the head of the femur had mounted high enough to occasion notable shortening, which persisted after the operation.

The wound only requires simple treatment. The edges are drawn together at the two angles by bands of diachylon, while the middle is left open for the introduction of a few balls of lint. The whole is then covered with anointed linen, and with compresses. This treatment may continue until the wound is filled up with fleshy granulations. If the suppuration is abundant, the wound should be washed two or three times a day with warm aromatic injections; if there be danger that it close too quickly, a caoutchouc drainage tube is introduced.

Before cicatrization is complete, it is well to accustom the limb to some slight movements, but only allowed gradually and with much caution. These movements are renewed and extended in different directions, so as to restore, if possible, mobility to the joint. Excessive exercise, however, is hurtful, as tending to produce too much laxity in the articulation.

In the two most recent cases of resection, the patients preserved the mobility of the femur, and recovered with a pseudoarthrosis instead of an anchylosis. In Mr. Le Fort's memoir on the subject, twenty-seven patients are reported to have escaped

with a perfectly useful articulation, and capable of walking very tolerably, although more or less lame.

Permanent shortening of the limb is to be palliated, of course, by a raised metallic sole to the foot.

Difficulties to Childbirth in Abnormal Narrowness of Pelvis.

A curious calculation is made by Dr. Vignard in relation to the difficulties opposed to childbirth by abnormal narrowness of the pelvis. The reflection is suggested by a case occurring in his practice, in which the sacro-pubic diameter of the basin was eighty-nine millimetres. All attempts to deliver the child by forceps proved unavailing, and the accoucheur was obliged to have recourse to craniotomy.

The woman had already had three children, and according to the husband's account, the first two, though delivered with forceps, came into the world alive and well, and were still living. The third, he admitted to have been born dead, but was still delivered with forceps. All three were girls. After the patient had recovered from the effects of the labor (lasting forty-eight hours), and of the operation, the physician requestioned the husband, and ascertained that, in truth, craniotomy had been performed upon this last child. It was not surprising, therefore, that the fourth, which was a boy, should have required the intervention of the fatal operation.

Hence the obstacle to delivery had continually increased with each successive birth. There was no reason to attribute this increase to any greater narrowness of the basis, but rather to what Dr. Vignard asserts to be a well recognized law, namely: that a woman's first children are always the smallest, and the size increases with each new birth. Thus, in this case, the first forceps delivery had been easy, the second difficult, the third accouchement required craniotomy, and finally, in the fourth, the masculine sex of the child introduced another cause of increased size. As a practical rule, therefore, Dr. Vignard recommends, whenever an abnormal retraction of the pelvic cavity has been discovered, sufficient to require the forceps to draw the fœtal head into the superior strait (of course, the application of forceps for any other reason would not count), and especially when craniotomy had once been practised—in these cases he recommends when a new pregnancy occurs, that premature delivery be

provoked at the eighth month. For it may be regarded as certain, that whatever difficulty has already existed, will be presented again, and in a more formidable degree, and that a woman who has once lost a child by craniotomy, can never hope for living offspring, if she waits till term to be delivered.

Cases of Complicated Rheumatism.

M. Boucand, of Lyons, reports several cases of grave rheumatism, severally complicated with pneumonia, albuminuria hæmorrhage, or encephalic accidents.

In the first case, the patient was a man about 40 years old, and when first observed, after an illness of eight days, was in a demi-typhoid condition, manifested by general prostration, slowness of speech, dry lips, cracked tongue, great thirst, and slight epistaxis; but without any eruption. The pulse was vibrant, and at 130—slight cough, mediocre oppression, tubular breathing, and bronchophony at the summit of both lungs. The patient gave no sign of sensibility, except when his right thigh was touched or extended, when he screamed out. A rude bellows sound was heard at the base of the heart.

The patient was thus affected at once with double pneumonia, endocarditis, arthritis of the right hip-joint, burning fever, and stupor. According to M. Boucand, all the other conditions were under the dependence of the abnormal rheumatism. The patient succumbed on the second day, but no autopsy could be obtained.

The second patient was a woman of 34 years, admitted to the hospital with acute rheumatism, complicated by endocarditis. After admission, she was attacked with pleurisy, accompanied by very moderate effusion. The urine contained albumen at this time. The arthritis persisted at the knee and wrist, in spite of the pleurisy. Suddenly, the patient, who suffered from insomnia, but whose cerebral functions remained intact complained of excessive oppression, and sibilant and subcrepitant râles appeared in all parts of the chest. The patient died 36 hours after this invasion of pulmonary œdema.

The third observation relates to a man 25 years old, attacked with acute rheumatism for the third time. He labored under arthritis of several joints, intense fever and sweating, and repeated and abundant epistaxis. The skin was covered with

sudamina; a soft, blowing sound was heard at the base of the heart; bilious vomiting occurred several times.

This patient recovered in 25 days, without preserving any sign of cardiac disease.

At the same time was received at the hospital a woman four months advanced in pregnancy, attacked with polyarthritis and endocarditis. She was affected also with epistaxis and also spitting of blood. The skin was red, and covered with sudamina. This patient was seized with eclamptic convulsions several hours before dying. No autopsy could be made.

In the fifth case, a lymphatic girl, aged twenty-two, was treated in September for acute rheumatism, and left the hospital, cured of the acute disease but in cachectic condition, and subject to diarrhœa. She returned in a month, complaining of intense cephalalgia. Vomiting, nocturnal delirium, contraction of the maxillæ and the muscles of the neck came on, and the patient died ten days after admission.

Finally, another woman, 46 years old, was admitted on account of general feebleness and leucorrhœa, unaccompanied by organic uterine lesion. She suffered from no heart symptoms; but a well characterized organic disease was discovered, and the patient acknowledged having had several attacks of rheumatism. The patient was put upon digitalis and a tonic course of treatment; when, a week after her entrance, new symptoms suddenly declared themselves, beginning with moderate fever, complete stupidity, and anæsthesia and hemiplegia of the left arm. The left side of the face was paralyzed, the tongue deviated; no reply could be obtained to questions; complete prostration of strength, and loss of appetite. The urine contained albumen. Four days afterwards arthritis of the right wrist declared itself, and immediately the intelligence returned, and the left arm recovered its motor power. A fortnight later, the albumen had disappeared from the urine, the intelligence remained intact, there was no more sign of paralysis, and the patient ultimately left the hospital in a very satisfactory condition.

In connection with these two cases of meningitis (for so M. Boucand feels entitled to call them), produced under the influence of rheumatism, the writer reports several cases of adult meningitis occasioned by other diseases. In one case it was a pneumonia, occurring in a person addicted to intemperance. The meningitis

declared itself during convalescence from the original disease, and at the autopsy the pneumonia was found to be in full course of resolution; but a soft exudation had developed between the arachnoid and pia-mater. In another case the cerebral disease came on during an anomalous variola, where the eruption was late and scanty, consisting at first of herpetiform vesicles. At the autopsy a layer of greenish pus, infiltered under the arachnoid, was discovered on the upper surface of the cerebellum and the inferior extremity of the spinal cord. In a third case, an erysipelas of the scalp was the primitive affection, but when delirium declared itself, the opinion of Trousseau, who declares this symptom to be insignificant in the course of this disease, invested the prognosis with an optimism which the autopsy of the patient thoroughly routed, for the signs of meningitis were evident. Finally, is a case of meningitis occurring during typhoid fever. The patient was nineteen years old, and died the 20th day of the disease. She had coma and stupor, dorsal decubitus, fall of the eyelids, deafness, cephalalgia, dilatation of the right pupil without strabismus. The pulse was 100, the skin dry, and the temperature thirty-eight in the armpit. The jaws were so forcibly contracted as to render examination of the tongue impossible. Sensibility of the skin remained sufficiently keen; there was iliac gurgling, and retention of urine with distension of the bladder. A certain amount of contraction existed in the muscles of the neck and back; the thorax was sonorous on percussion, but there were sibilant and crepitant râles, constipation persistent, and vomiting of liquid ingesta. Three days before death hallucinations occurred, with contraction of the wrists and carphology, and the patient ceased to recognize her mother. Several attacks of epistaxis and tracheal râles preceded the death, which occurred in the midst of a continually increasing dyspnœa. The urine evacuated by the sound was red and extremely fœtid.

This case is extremely interesting from the curious mixture of the symptoms severally characteristic of the two diseases which found themselves in presence, and from the manner in which the meningitis gradually obtained the ascendency over the fever, so that at last it seemed to rule alone. But at the autopsy, the reality of sloth in enteritis was well demonstrated by two grayish ulcerations in the ileum and at the ileo-cœcal valve. The meningitis was evidenced by a sero-albuminous effusion in

the anterior subarachnoidal space; by the thickening and vascularization of the pia-mater, everywhere adherent to the brain; by adhesion of the two cerebral lobes at the fissure of Sylvius; by half a glass of thick whitish liquid, like whey, in the third ventricle. Neither pus nor tubercle nor gray granulation along the arteries could be found.

M. Boucand remarks, that primitive meningitis is so rare with adults that, in presence of acute meningical disease, search should always be made for some other malady which has served as its cradle.

Nux Vomica in the Dyspepsia of Hypochondriacs.

En Fait de Therapeutique.—There are, as always, one or two items or suggestions worthy of being placed in the budget. Professor Trastour, of Nantes, has occasion to highly praise the employment of nux vomica in all forms of atonic dyspepsia, and especially as a relief for the painful digestions so common among the hypochondriacs. His theory is based upon the two facts, that nux vomica stimulates and regulates the activity of the spinal cord, especially in regard to its reflex action, and that the integrity of the functions of the grand sympathetic is subordinated to the regular accomplishment of the functions of this part of the nervous system.

The following is a useful formula:

℞.—Pulv-nux vom. 1—4 grammes.
Pulv. cassiæ ligneæ 2 "
Carb. calc. or carb. mag. 2 grammes.
M.—ft. pulv. 20.

One powder at the beginning of each meal, in unfermented bread.

M. Trastour, like many of his confrères, prefers nux vomica to the salts of strychnine, both on account of its innocuousness and its efficacy in dyspepsias.

Sulphide of Carbon as a local Anæsthetic.

Recent experiments have been made upon the properties of sulphide of carbon as a local anæsthetic, and have been very satisfactory. The cold induced is more disagreeable than by the volatilization of chloroform and ether, but the analgesia is more

profound. A splinter of wood, encysted since two months, was easily removed under the influence of the sulphide of carbon, after the operation had been abandoned on account of the pain which persisted in spite of the local application of ether.

Dextrine in Varicose Eczema.

Finally, a suggestion in reference to the treatment of that obstinate disease, varicose eczema, cannot be inappropriate. It is recommended that the limb be swathed in linen bands, previously dipped in a solution of dextrine, made with 125 grammes of dextrine to a litre of boiling water. Compresses, dipped in the same liquid, should be laid upon the limb previous to the application of the bandage. This is then allowed to dry, and only renewed when it tends of itself to unroll—that is, by the fourth or fifth day. The eczema should have become tolerably dry before this application can be indicated.

M. Dévergie, whose name is of such authority in skin diseases, finds that his patients are infinitely better off with the dextrined bandage than with the laced stocking. The bandage is useful even without the dextrine, but the addition of this latter prevents the linen from becoming soaked with liquids, in which case it can hardly be removed without tearing a considerable portion of the epiderm. P. C. M.

The Theories of the Dermatologists of the Hôpital St. Louis.

To the Editor of the Medical Record.

Sir—Who, from the commander-in-chief to the smallest corporal in the vast army that wages war on disease, has not heard of the Hôpital St. Louis? Who, that makes the most flying visit to Paris, fails to contrive an excursion to its somewhat distant locality, for at least one walk through its great cool wards, through the brilliantly clean courts, and spots of refreshing garden? The whole forms a little city in itself, where the most repulsive forms of disease, assembled in an immense collection from all corners of Paris, and Europe, and the entire world, are stripped of a large share of their deformity by the influence of their surroundings, and a large share of their gravity by the enlightened skill of the brilliant coterie of physicians who make St. Louis the arena of their conflicts and triumphs.

These conflicts, it must be confessed, are not exclusively carried on between the doctor and the bodies, or skins, of his patients. It is often a war *intra muros*, a rivalry of opinion among the physicians, which is sustained with such vehemence, that the spectator asks himself nervously, what would be the consequence if the chiefs of opposing camps should encounter each other in the morning at the narrow stairway that opens into the Hospital grounds? The meeting would be more critical than that of Raphael and Michael Angelo in the Vatican, and, perhaps to avoid its chances, the different physicians seem to arrange to arrive at their respective wards at different hours.

The names that are at present associated with the promulgation of any special doctrine, are those of Cazenave and Giebert, Dévergie, Bazin, and his somewhat wavering satellite, Hardy. The two first are devoted pupils of Bielt, who himself was an ardent disciple of Willan and imported the classification of the English dermatologist at the very moment that in another ward at St. Louis, Alibert was proclaiming his,—and planting his *arbre des dermatoses* in the imaginations of an entranced audience.

Their system, therefore—and I believe it is that best known in America—considers exclusively the primitive anatomical elements of cutaneous eruptions, classified as: 1st, Exanthemata; 2d, Vesicules; 3rd, Papulæ; 4th, Bullæ; 5th, Squamæ; 6th, Tubercles; 7th, Pustules; 8th, Maculæ. Gibert classes lupus elephantiasis, and several other exotic diseases, under the head of Tubercles; Cazenave makes of each of them, as well as Purpura and Pellagra, a class apart; and both recognize the Syphilides as a separate class.

This system is certainly characterized by an extreme simplicity, even an ostentatious absence of all pretension to theory or doctrine. Distrusting their own ability to discover any connecting links between the multiple phenomena of skin diseases, the authors confine themselves to making a simple statement of such phenomena, considered as purely local affections. In a volume published this year, M. Cazenave reiterates substantially his old principles, makes light of causes of diseases, which constitute the basis of Wilson's classification, entirely rejects scrofula even as an influence in dermatology, and only improves upon his original programme, by the introduction of certain researches into Pathological Anatomy, which, unfortunately, are more often hypotheses

than researches. Thus he declares eczema to be an inflammation of the sudoriferous glands; impetigo, an inflammation of the lymphatic vessels; lichen, irritation of the papillæ of the derma; but brings no microscopic proof of his assertions, which are more or less plausible. Bazin admits the probable lesion of the sudoriferous glands in eczema. Dévergie acknowledges that impetigo is generally grafted upon a lymphatic temperament; but Hardy observes that the papulæ of lichen and prurigo do not bear the slightest resemblance to the normal papillæ of the derm, either in their distribution, which in nowise recalls the regular concentric lines of the papillary stratum.

In view of the double difficulty in the way of anatomical researches occasioned by the infrequence of mortality from skin diseases, and their cessation at the occurrence of any serious malady, it may be questioned whether, with the assistance of local anæsthetics, a physician might not extirpate from the skin of a living patient such a minute segment as would be needed for microscopical examination. Many obscure points would thus stand a chance of being elucidated.

Besides this self-restriction to the anatomical characters of skin diseases, M. Cazenave is further noticeable (and especially in his recently published work on General Pathology of the Skin) for an entire rejection of vegetable parasites as intervening even in favus. Herein he is in complete accordance with the English dermatologist, Wilson. Now the rival school, composed of M. Bazin, supported by M. Hardy, and moderately admired by M. Dévergie, is distinguished by its extensive adoption of cryptogamic etiologies, by its discontent with "lesions" of the skin, as the ultimate explanation of its diseases, and by the research after general constitutional causes for all affections that are not parasitic in their origin. The theories of Bazin have been for some time on the carpet, but as I believe that they have not widely circulated on the other side of the water, and as they are extremely interesting, and, if true, extremely important, I will expose them in some detail.

The starting-point of the theory is to be found in the generally acknowledged existence of the great class of Syphilides, affections, which, though embracing the entire range of primitive anatomical elements, are all distinguished by characteristic features; copper color, circular form, white *liséré* (called Britt's, from the emphasis

he laid upon this desquamation of the epiderm around a primitive element); blackish green crusts, grayish ulcerations with sharp indurated edges; smoothish but indelible cicatrices, etc. In this case the elementary *lesion* is common to specific and non-specific forms of disease; the *affection*, formed by the grouping of the elements, as an ecthyma, from pustules, ulcers, and crusts, is generally common also, although some forms are almost exclusively syphilitic; but the *malady*, the general constitutional conditions upon which the affections depend, and which give them their significance, alone are separate and peculiar, alone assume a distinct individuality, requiring a distinct therapeutic treatment. So imposing is this individuality, that it overpowers all other considerations; should microscopic analysis demonstrate absolute identity between the lesions of specific and non-specific eruptions, the prognosis and therapeutics of these latter would remain none the less dependent upon the diagnosis of the constitutional disease.

Setting out from this universally acknowledged doctrine, M. Bazin has inquired if the great class of non-specific eruptions might not also be brought under the influence of constitutional diseases. The result of his researches has been the integration of three great maladies, whose individuality is as distinct, and relation to cutaneous affections as important, as those of syphilis. These maladies are, Scrofula, Dartre, and Arthritis, and I name them in the order in which they have gained public credence. Dévergie admits scrofula; Hardy, scrofula and dartre; Cazenave and Gibert deny even scrofula as regulating skin diseases; finally, only the pupils of M. Bazin believe in the existence of arthritis.

M. Bazin gives the following definitions of *Maladies*, *Diatheses*, and *Affections:*— "A malady (or disease) is a state of the body which produces functional disorders, called symptoms, or material disorders, called lesions. *A constitutional disease* is a malady, acute or chronic, pyretic or apyretic, continued or intermittent, contagious or non-contagious, *characterized by an assemblage of morbid products, and of extremely varied affections, attacking any or all the organic systems.*

A *diathesis* is a malady, etc., characterized by the formation of a *single morbid product* that may be deposited in any or all of the organic systems. Tuberculosis and cancer are examples of dia-

theses, and several others are admitted, the hæmorrhagic, saccharic, fatty, etc.

An *affection*, is what we commonly call a *disease* of any particular apparatus, as the skin, and corresponds to an assemblage of elementary lesions and symptoms, of which, however, it is not the *cause* but the statement. The cause resides in the constitutional disease.

The problem of the diagnosis of any cutaneous disorder is, therefore, threefold. It is necessary to determine: 1st, the anatomical element, as for instance the vesicle as distinguished from papulæ, pustules, etc.; 2d, the affection, as an eczema, distinguished from herpes, scabies, or other vesicular affections; 3d, the nature of the disease of which the affection is the expression for the time being—whether for instance, the eczema be scrofulous, or dartrous, or arthritic. Each case is characterized: 1st, by objective peculiarities proper to the affections of each constitutional disease; 2d, by the coincidence of general symptoms, equally characteristic of such disease, even in the absence of a cutaneous affection.

In the diagnostic of constitutional scrofula, M. Bazin does not greatly differ from the majority of physicians, with whom it is, of course, the most usual thing in the world to admit a scrofulous constitution, and to consider that it impresses a certain character upon *some* eruptions. Only Bazin calls a *disease* what others only name a *tendency* to disease; the engulphs the lymphatic temperament (upon which Dévergie greatly insists as predisposing to scrofula) with scrofula itself, and he entirely rejects Cazenave's restriction, for whom chronic inflammation, or tubercular degeneration of the lymphatic glands, constitutes the sole expression of scrofulous disease.

Assuming an exact parallel between the evolution of syphilis taken as a type, and all other constitutional diseases, M. Bazin divides scrofula into four periods, each characterized by peculiar affections, and the two first by special affections of the skin.

First Period.—Mild cutaneous disorders, including *Gourmes* (which Cazenave regards as accidental, and Dévergie as depurative), eczema, impetiginous eczema, impetigo; also dry scrofulides, erythema, prurigo, lichen, psoriasis, acné simplex in all its forms including acné sebacea; scrofulides of the mucous mem-

branes; habitual coryza, seropurulent otorrhœa; glandular blepharitis; dacriocystitis, with lachrymal tumor and fistula; scrofulous ophthalmia and keratitis; reiterated bronchitis; amygdalitis, stomatitis; certain tenacious diarrhœas; certain inflammations of the vulva and vagina.

For all these affections, as will presently appear, with the exception of gourmes of the head in young children, M. Bazin admits the existence of other forms more dependent upon other diseases than scrofula.

Second Period.—Profound cutaneous affections leaving cicatrices; lupus, both the erythematous and tuberculous variety; papulo-pustular scrofulides; impetigo rodens; certain serious forms of acné, molluscum; also more obstinate affections of the mucous membranes, leucorrhœa, with erosions and granulations of the neck of the uterus; blennorrhagia with enlarged prostate, and urethral stricture.

To the *Third Period* belong affections of the bones and articulations, and to the *Fourth* visceral and parenchymatous lesions generally tubercular, with *Hectic* absent or slightly marked, although Bazin admits a tubercular diathesis independent of scrofulous disease. You instantly appreciate the difference between this view and that which takes into account scrofulous constitution and scrofulous *diseases*, but not *a* scrofulous *disease*, with a regular evolution and distinct degrees and stagings. Of the three (scrofula, dartre, and arthritis), it is scrofula that approaches most nearly to the standard type, but even here the critic is forced to object that M. Bazin often strains his analogy beyond the warrant of facts. Tertiary syphilis never occurs without having been preceded by primary and secondary symptoms; while in scrofula, however frequent may be the instances of preliminary eczemas, impetigos, etc., M. Bazin himself admits that a cicatricial scrofulide, a lupus, may declare itself in a subject who has never suffered from any previous eruption. In this case he would claim that the links are supplied by some ganglionic scrofulide—some blepharitis,—and urge the example of syphilitic patients who suffer from osteocopic pains after the engorgement of the lymphatic glands has taken place, but without having exhibited any roseola, papulæ, etc.

M. Bazin of course does not pretend to make all the terms of scrofula correspond to those of syphilis. The initial infection

lacking in the first disease, the affections of its first and second periods, correspond to those of the second in the syphilitic malady. The third and fourth periods resemble each other in the two diseases.

Comparison of the objective characters of scrofulous affections is best made after the description of the two remaining constitutional diseases.

Dartre, as you know, is an old French word, formerly employed to designate all eruptions except those of the head, which were similarly huddled together under the name of *teignes*. Alibert retained the term, though greatly modifying its acceptation. Bazin, followed by Hardy, has revived the name, and applied it, not to a *tendency*, a *habit* of body, but to a distinct constitutional disease, with regular march, evolutions, symptoms, etc. He has adopted as a synonym, though without clearly explaining why, the word *Herpetic*, as the general term to characterize eruptions dependent upon dartric disease. The evolution of the dartre is divided into four periods, preceded by more or less well defined

Prodromata.—These—that would be more justly entitled, Indications of a predisposition—consist in: scanty transpiration, skin dry, irritable, subject to ephemeral eruptions; thinness; frequent diarrhœa; nervous affections, sick headaches, gastralgia; a disposition irascible and melancholy.

The *First Period* is marked by the appearance of pseudo-exanthemata, urticaria and zona. Eczema also is of frequent occurrence at this stage.

Second Period.—Dry herpetides, psoriasis, pityriasis, lichen; secreting affections, eczema, dartrous impetigo, mentagra, pituite, blennorrhagia, leucorrhœa, rebellious diarrhœas; often ascites, and hydropericarditis, increased irascibility, often insanity.

Third Period.—The cutaneous affections tend to generalize themselves, and visceral disorders occur.

Fourth.—Extreme emaciation; infiltration of cellular tissue; skin clinging to the bones, covered with scales, crusts, and inflammatory exudations; hectic fever, death by syncope.

Of course the only cases where the symptoms of this fourth period are directly dependent upon cutaneous affections, are rupia, or pemphigus, and cachectic ecthyma. In all other

instances a visceral disorder has been induced, under the influence of the constitutional dartre.

Before commenting on the substantiality of this pathological entity, I will quote the parallel description of arthritis, the third darling of M. Bazin's brain, and the most dearly loved of all, for the very reason that it is entirely ignored by the rest of the world:—

Prodroma.—Exaggerated transpiration; tendency to obesity and development of the muscular system; constipation, hæmorrhoids, sick headaches, congestion of the head, epistaxis, vertigo, ringing in the ears.

[This description applies evidently to persons with "a full habit," and subject to the inconveniences and consequences of constipation.]

First Period.—Articular rheumatism; eczema of the scalp (before puberty, afterwards it is more disseminated); erythema of the external organs of generation; œdematous erythema around the articulations; urticaria, zona, herpes, acute pemphigus, furuncles and anthrax; coryzas, bronchitis and ophthalmias; sick headaches and arthritic dyspepsia; vague muscular pains.

Second Period.—Attacks of gout and of acute articular rheumatism; cerebral congestions, anginas, obstinate coryzas; dyspepsia with burning at the stomach, pyrosis, constriction of the œsophagus; localized pruritus, especially at the nostrils, anus, and genital organs; sometimes anal fissure.

Third Period.—More serious lesions of the articulations, tophus, destruction of cartilages, caries of bones, ankylosis.

Fourth Period.—Organic affections of the heart; congestions and apoplexies; catarrhal asthma; various lesions of the liver and kidneys.

Although M. Bazin, in this extensive generalization, unites gout and rheumatism like a pair of Siamese twins, he by no means claims their identity. They both come under the great class Arthritis, but possess their individual and distinguishing characteristics. In the same way he attaches cardiac affections, not to rheumatism itself, as generally acknowledged, but to a more general condition, that embraces the two.

The elaborate specification—which I shall presently expose—by which affections, similar, but belonging to different con-

stitutional diseases, are distinguished from one another, will bring out into much stronger relief the peculiarities that are supposed to characterize these diseases, than this succinct generalization is able to do. But with that alone before us, we can fairly criticise its basis, to the extent to which that is independent of the cutaneous disorders it professes to explain. Assuming—as I think can be proved—that cutaneous affections vary in four principal modes, conveniently designated as syphilitic, scrofulous, herpetic, or arthritic, and that to each of these modes is attached, more or less loosely, a liability to certain disorders affecting other parts of the economy,—we are not therefore obliged to conclude that each mode constitutes a disease, which constantly holds the patient in its clutch, menaces him even at moments that the temporary absence of all affections seems to leave him in perfect health, and can never be considered cured until it has completed its entire evolution, and been subdued at the fourth period. The proof of the existence of such a disease would be found in the regularity of its evolution, the absence of interversions, the constant reunion of a *sufficient* number of characters to establish its identity. Already scrofula—the nearest approach to the type exhibited by syphilis—begins to fail in some of these requisitions. (We mean of course scrofula in relation to cutaneous affections, not the type, glandular scrofula, which often has nothing to do with them.) Undoubtedly numerous instances exist, as I have had an opportunity of observing at St. Louis, where M. Bazin's descriptions are strikingly verified. But numerous exceptions exist also. Patients will suffer for years from rheumatism, even in its gravest forms, and then exhibit an eruption belonging to the first period of arthritis—a circumstance as embarrassing to the theory as if a gummy tumor should be followed, instead of preceded, by a chancre! Others develop so called constitutional affections, after a lifetime of perfect health, and I have observed that whenever M. Bazin has to do with a remarkably robust patient, who seems to have never exhibited a morbid symptom, he generally ranks him under the head of arthritis. Again, rheumatism is as frequent among thin, weakly people, who from their appearance ought to belong exclusively to the domain of Dartre, as to the constipated, obese individuals whom M. Bazin considers to be alone entitled to its afflictions. In this particular, the theory seems affected rather

by old prejudices than by modern ideas. Again, without being so exigent as to demand that every patient should present the entire *cortége* of symptoms proper to his disease, we are at least entitled to expect the presence of a certain number, upon which to establish a diagnosis. Yet M. Bazin will sometimes claim an arthritis from the fact that the patient's father was subject to sick headaches; or a dartric because the patient has a bad temper, and suffers neither from hæmorrhoids nor constipation; or a scrofula, because the skin is white and the temperament lymphatic. In a word, M. Bazin, like all systemizers, being possessed of a vivid imagination, and a despotic resolve to subdue all facts to his system, refuses to admit that any diseases, with the exception of certain exotics, and the parasitic class, can exist except as dependencies of one of his four great classes, and consequently strains his theory, which, if left in its proper place, would be infinitely stronger and more valuable. For, having made all these deductions, and having changed the too absolute word, disease, into the more usual and acceptable term, diathesis (to which M. Bazin gives a forced and arbitrary signification), there remains an acute and suggestive generalization—which, as we shall presently see, affords much practical assistance in the comprehension, diagnosis, and treatment of diseases of the skin. M. Bazin has not condescended to support his views by statistics, but the suggestion might usefully set other people to work, to search for confirmation or condemnation of the theory. An inquirer, who admits that cutaneous affections may be either accidental or constitutional, will be less embarrassed in the establishment of constitutional influences, than if obliged *mal gré bon gré*, to drag every eruption under such influence; and the relations between rheumatic and cutaneous diseases may be more clearly discerned, and the dartric diathesis, admitted in France from time immemorial, more precisely determined, by the observer who was not self-compelled to prove a regular evolution of a disease where facts only warranted the irregular connection of affections—rooted, not in a malady that had possessed the body, but in the innate tendencies of the body's tissues and component parts.

In my next letter I will describe the objective diagnosis, made out in obedience to M. Bazin's theory, but capable of application even by those who only admit this theory with modi-

fications, and this will tend naturally to a brief notice of the therapeutics of St. Louis. P. C. M.

PARIS, Aug. 21.

The Pathology, Diagnosis, and Treatment of Skin Diseases.

To the Editor of the Medical Record.

SIR:—The most remarkable instance of the application of Bazin's theory in the diagnosis of cutaneous affections is afforded by eczema. According to Hardy, this is always *dartrans*, and always—in its chronic forms at least—to be treated by arsenic. For Dévergie it is a constitutional disease, belonging to no particular diathesis, but expressing a depuratory effort of nature, and consequently must not be cured, especially in children. Cazenave pooh-poohs this ideal, as in fact do almost all the other physicians, and declares eczema to be an accidental affection, whose principal characteristic is expressed in saying that it is a lesion of the sudoriparous glands. But M. Bazin divides eczematous affections into three great classes, belonging to scrofula, arthritis, and dartre. Syphilis is represented by vesiculous eruption, or false eczema. Each affection requires a different treatment.

Urticaria, as a manifestation of dartre, is rather pale, and occurs under the influence of moral emotions. The arthritic variety is deep red, and occasioned by cold and gastric disturbance. It frequently complicates rheumatism, springing as it does from the same diathesis. The same distinctions may be made for *acute pityriasis*, as when arthritic complicates rheumatism, when herpetic is accompanied by sick headaches, and determined by moral emotions.

In arthritic *herpes* is noticeable the inequality of the vesicles, already mentioned as a characteristic of eczema. M. Bazin adds—but very inappropriately—the peculiarity of occupying uncovered parts of the skin. He thus passes over the well-known fact that herpes labialis is an extremely frequent complication of fevers (other than typhoid), and thus may be found on individuals of all constitutions. The herpes of children is certainly much more frequently under the influence of dartre or scrofula than of arthritis, which rarely manifests itself at an early age.

Herpes circinatus is regarded by all the St. Louis physicians, with the exception of Cazenave, as a parasitic disease, originating in a cryptogam, identical with that producing herpes tonsurans of the hairy scalp, and sycosis of the beard. The cryptogam is named tricophyton, and M. Hardy classes these three diseases together as one, tricophyte.[1]

In herpes zoster, same distinctions in regard to the vesicles and antecedents; besides, the pains in arthritic zona are burning, deep-seated, muscular, and often disappear with the appearance of the eruption. In herpetic zona (that is, under the influence of the dartre), the pain is lancinating, neuralgic, and generally lasts after the eruption has disappeared.

In herpetic chronic pemphigus, the bullæ contain a transparent citrine colored fluid, are isolated, and equal in size. In the arthritic form, the bullæ are sero-purulent, unequal, and united in large erysipelatous patches. But the cachexia of pemphigus is the type of what M. Bazin calls the cachexia of dartre, arrived at its ultimate term, and this is true, however the *début* may be characterized by slight differences. *Acute pemphigus*, according to Hardy, is merely an accidental erythematous eruption, the bullæ being quite secondary to the erythematous patches upon which they appear, like the phlyctenæ in erysipelas.

A curious case of this affection entered M. Hardy's ward the other day. The patient was a man just recovering from an attack of lead colic, and had been similarly affected with pemphigus at the same period of a previous convalescence from the same disease. On admission, he was as red from head to foot as a boiled lobster. The fiery patches were not absolutely coalescent, but so nearly so that the effect was almost as vivid. Upon the greater number the epiderm was elevated in bullæ of different sizes. A slight febrile movement accompanied the eruption. It was treated like an eruptive fever, let alone, and in a week had almost entirely disappeared, leaving brown stains in the place of the patches, that in their turn faded rapidly. The affection was therefore essentially distinguished from real pemphigus, by expending itself in a single eruption, whereas the more formidable disease is noted for the desperate

[1] Excuse me if I have mentioned the above idea somewhat dogmatically, as if on the supposition that it was entirely unknown at home. But Wilson does not mention this theory, and Cazenave disputes it, and as their works are the best known in America, I have ventured to be somewhat explicit.

tenacity with which fresh crops of bullæ continue to appear. M. Bazin has a little pet variety of chronic pemphigus—invented by himself—and entitled, Hydroa with little bullæ. This pretty name is applied to a variety almost as chronic as the ordinary form, but affording infinitely greater chance for cure. In fact, whenever a patient affected with pemphigus gets well, M. Bazin calls his malady Hydroa. But the objective feature is the small size of the bullæ, some of which are not larger than vesicles. They appear successively, but only one or two at a time, and are covered by very thin crusts. The general health of the patient does not suffer the deterioration so remarkable in ordinary chronic pemphigus.

Bazin professes to distinguish psoriasis, lichen and prurigo into arthritic and herpetic varieties, but the line is not very well defined. He observes that the old herpetic affections are always symmetrical, the arthritic almost always irregular, and not extending in large confluent patches. Certain horse-shoe and circular forms of psoriasis, with a shining coppery hue to the skin, instead of the raw ham look of the non-specific variety—belong to the cutaneous manifestations of syphilis. The diagnosis is evidently of the utmost importance. Besides these affections, containing varieties that belong either to anthritis or dartre, are certain others, peculiar to one of these constitutional diseases. Erythema nodosum, and papular erythema, are both arthritic, according to M. Bazin. So also, St. Anthony's fire, or *couperose*, which he carefully distinguishes from the acne rosacea, of which it is a frequent complication. But the pustules belong exclusively to the acne; the couperose is constituted by dilatations of the cutaneous capillaries. Acne is always either arthritic, or scrofulous, or syphilitic, never herpetic. The scrofulous acne develops on the face; the arthritic affection (except the indurated form, which is facial and distinguished from scrofula, principally by the antecedents, &c.), appears on the back and shoulders; syphilitic acne is disseminated all over the body, and noticeable by the characteristic color of the areola, and the fine epidermic scales that remain after desiccation of the pustules. M. Bazin insists upon this latter sign, and in his clinic mentions cases where its absence has served to correct diagnoses of syphilis obstinately applied to young persons whose character was above reproach. Mentagra may be arthritic or scrofulous

(scrofulous sycosis), but is not herpetic. The arthritic eruption consists of pustules, seated on indurated tubercles, which occupy nearly the entire thickness of the derm. The eruption is in circumscribed patches occupying the beard on the chin, cheeks, or naso-labial sillon. The crusts are thin, brown, and broken. In scrofulous sycosis, the crusts are yellow, thick and moist, the lips are swollen, and the face generally occupied by acne indurata. The tubercles are more superficial.

Syphilitic acne of the beard, which closely resembles the mentagra, is still more profound than the arthritic variety; the sub-cutaneous cellular tissue is inflamed and indurated. Finally, parasitic sycosis, constituted by the same cryptogamic vegetation as produces herpes tonsurans (the tricophyton), is noticeable for the alteration of the hairs, which become gray and lustreless in color, broken irregularly, and covered over the roots with a fine gray powder. On the other hand, nonspecific roseola is always considered as a manifestation of dartre; one of its early symptoms, as papular erythema of arthritis. A certain form of impetigo, which M. Bazin calls melitagra, is ranked as herpetic, and distinguished from scrofulous impetigo—which occupies the head, and appears in large confluent patches—by appearing symmetrically on the trunk and limbs, in psydracious pustules more or less isolated.

M. Hardy calls impetigo simply the second stage of eczema, of which pityriasis is the third, sometimes also the initial period.

Finally (for M. Bazin's dissertations on syphilis do not greatly differ from those of the rest of the world, and may be left out of the question), the great class of scrofulides, divided into benign and malignant, occupy the third place in the category of affections dependent on constitutional disease—the first, perhaps, in importance. It is unnecessary to repeat the symptoms of general scrofula, recognized by everybody. M. Bazin errs, perhaps, in absorbing into scrofula the *lymphatic temperament*, which, though tending towards scrofula, is distinct from it. As symptoms of the first period of scrofula, he reckons the benign scrofulous eruptions, classed as exsudative, erythematous, and papular, and distinguished from the malignant scrofulides by leaving no mark or cicatrice. In the first class (exsudative), are red gum, eczema, impetigo, and acne sebacea. The characters distinguishing eczema and impetigo have been mentioned

above. All forms of sebaceous acne, whether fluid or concrete, are recognized as scrofulous. An interesting case of this troublesome affection was admitted to M. Bazin's ward a little while ago. A girl of sixteen, florid and stout, without, however, any appearance of scrofula elsewhere, or any derangement of health, had been unsuccessfully treated for a year in the attempt to remove a patch of concrete sebaceous matter, about three centimetres long, situated just above the right eyebrow. If this were scraped away it immediately reproduced itself, and constituted a disagreeable deformity, being a thick, yellow, unctuous mass, clinging like a plaster to the forehead. Treatment by cod-liver oil and daily *alkaline* baths, removed the patch entirely, but the patient declared, from former experience, that it will return as soon as the treatment was interrupted. She is still at the hospital.

The papular scrofulides are somewhat discutable. Strophulus is certainly often an accidental affection. *Prurigo mitis*, with large papules, and only a supportable degree of itching, is considered scrofulous, while prurigo ferox always belongs to dartre. Erythema papulatum can be distinguished by no objective characters from arthritic erythema. I have mentioned above that facial acne was always scrofulous, that is, the varieties simplex and punctata, and occurring in young persons.

Among erythematous scrofulides M. Bazin counts chilblains; especially those accompanied by deep-seated chronic inflammation of the subcutaneous tissue,—locality affected in preference by the scrofulides.

The following are the characters common to all the benign scrofulides:—Tenacity, persistence in the same place (in opposition to dartre, so noticeable for its mobility); *début* by the head, gradual extension to the ears, face, and body; inflammatory process secreting, suppurating or hypertrophic: participation of the lymphatic glands, and subcutaneous cellular tissue; absence of pain or of intense itching. This latter circumstance is due to the deep seat of the inflammation. If it chance to be superficial itching becomes quite intense.

The malignant scrofulides (which, according to Cazenave, are all manifestations of hereditary syphilis) are remarkable for their extension to the deep layers of subcutaneous tissue, for their well-defined limits, and persistence in one place, for the absence of all

pain or itching, and for a strong tendency to relapse after cure. These eruptions are divided into three classes: ulcero-crustaceous, tuberculous, and erythematous. The crustaceous scrofulide contains two important varieties, inflammatory-ulcerating, and ulcerating with fibro-plastic formations. The first commences with tubercles or pustules simply inflammatory, which degenerate into ulcers, that destroy surrounding soft parts, but are arrested by the bones. These ulcers cover themselves with thick, green crusts, imbedded in the skin, and formed of superposed and concentric layers. Impetigo rodens and rupia are here included. After the crusts have fallen, and the ulcers healed, there remain white, irregular cicatrices, retracting the tissues like those of a burn, and adherent to the bones. In the second variety, the tubercles are fibro-plastic, caused by a proliferation of the cellular tissue, and the ulcers attack the bones as well as the soft parts. It is to this variety that M. Bazin especially applies the name of lupus vorax, which is considered an independent disease by some other dermatologists. He admits the title also in the second class, or tuberculous scrofulides. The primitive element is in this case the same as in the other, an inflammatory or fibro-plastic tubercle, but it remains stationary, without ulcerating on the surface. Curiously enough, however, the cicatrices are produced precisely as in the case of open ulcers, new fibrous tissue being called upon to fill up the place left vacant by the subcutaneous destruction of cellular tissue. Cure is only obtained at the expense of such cicatrices.

The same is true of the third class, erythematous scrofulides. These appear as a circumscribed patch of erythema, at first seeming to be as innocent as the ordinary ephemeral eruptions. But it presently reveals its real nature by its long persistence, its dull, pale red color, the pasty subœdematous feeling on pressure of the subcutaneous tissue, the absence of all burning, itching, pain or fever—finally, the appearance of a white irregular cicatrix in the centre of the patch, which gradually extends to the circumference.

Among erythematous scrofulides M. Bazin also includes the singular affection described by Dévergie as Herpes cretacea. In the case quoted by this latter writer, and which was, according to him, *mistaken* for an erythematous scrofulide, the disease began by an intense redness of the end of the nose, which persisted

with great tenacity; then the surface became furrowed, and from the furrows oozed a yellowish secretion, which hardened into thick, yellowish, prominent scales. The form of the patch was round, and it extended by new rings at the circumference.

Malignant scrofulides are distinguished from cancer, by the edges of the ulcers, which are undermined, instead of prominent, bosselated and indurated; by the bottom, which does not present the hard, fleshy granulations of cancer; by the *début* with several tubercles grouped together, instead of a single one, and by the complete absence of pain.

The diagnosis with syphilis is often much more difficult, since the eruptions in both diseases are painless, indolent, chronic, and composed of similar elements. But the syphilides are less chronic than the scrofulides; they date by months, but the latter by years. This is especially true of the erythematous scrofulide; a case in M. Hardy's ward now, has lasted ten years. In fact, there seems to be hardly any tendency to spontaneous cure.

Again: all forms of syphilides, ulcers, tubercles, or crusts, are surrounded by the characteristic coppery areola, and the tubercles are an obscure livid red. In the scrofulides there is frequently a bluish areola, and the tubercles are semi-transparent. Syphilitic crusts are blackish-green, and with edges detached from the skin; in scrofula, the color is clear green, and the crusts are firmly imbedded, often like a watch crystal in its case.

Exostosis and necrosis accompany syphilis; caries is produced by the eating ulcers of scrofula. Syphilitic ulcers are round, with characteristic edges, and grayish surface. The regular form is especially noticeable in the ulcers arising from gummy tumors, and in these the bottom is, in a number of stages, formed by successive growths of deep-seated gums. The scrofulous ulcers are irregular in form, the edges undermined, the bottom pale-red. Finally, syphilitic cicatrices are smooth, shining; scrofulous cicatrices irregular, and formed by the irradiation of innumerable retracting bands.

Scrofulous eruptions are much more frequent on the face; syphilitic affect the limbs, especially the lower ones; but, as is well known, often attack the face also, where they possess favorite localities.

A few words about the therapeutics of St. Louis, M. Bazin's treatment is in the main ranged under three heads: cod-liver oil,

iodide of iron, and sulphur baths for scrofulides, malignant or benign: alkalies, taken internally, and also in baths for the arthritides; arsenic internally, and saline baths for the herpetides. He declares sulphur to be positively injurious to the dartre, for which it has long been the popular remedy, and believes that its reputation is based on cures of scrofulides mistaken for herpetic affections. The use of alkalies, especially bi-carbonate of soda, for arthritis, seems to have been suggested by their employment in rheumatism, which the theory supposes to be akin to the eruptions in question. But the effect upon the cutaneous affections,—especially the influence of Eau de Vichy,—is often very remarkable. In other words, I have seen skin diseases, presenting the characters assigned by Bazin to the arthritides, treated perseveringly and unsuccessfully by arsenic, while similar cases in his wards recovered rapidly on the alkaline treatment.

The local treatment is pursued with great care, and comprises various resources. The actively inflammatory periods of all eruptions, as eczema, impetigo, pityriasis rubrum, and acute pemphigus, are treated by emollients, powdered with starch, or covered with cataplasms. These latter are applied also to indolent pustulous scrofulides, to remove the crusts. Only in zona and rupia, care is taken to preserve the crusts and vesicles intact, until the ulcer shall have healed underneath.

M. Hardy obtains extraordinary success by covering the eruption with vulcanized India-rubber. This retains the insensible perspiration, and keeps the part immersed continually in a natural vapor bath, which reduces inflammation, allays burning and itching, and removes incrustations in a very short time.

An India-rubber cap is often of signal service in eczema capitis with its tormenting irritation. In one case of severe herpetic eczema occupying both arms the rubber casing was at first applied to one only. In a week the scales had fallen, the secretion dried, the fissures to a great extent healed, and the angry redness was entirely subdued. The other arm, which only experienced the effect of the general treatment (tisane of wild violets and senna, acting as a derivative purgative, a favorite remedy with M. Hardy in the early treatment of all exsudative eruptions), remained in precisely the same condition as at first.

Vapor, cold and sulphur baths, and douches, are of course

largely included in the local treatment, but with about the same indications as are observed in other places than St. Louis. But many forms of disease are treated more boldly on a substitutive plan, than is generally the case elsewhere. Acne, for instance (which Hardy pronounces an accidental disease), is attacked by mercurial ointments and lotions of corrosive sublimate, with or without general medication.

Malignant scrofulides are painted with tinctures of iodine, ordinary or caustic, with oil of juniper, or of mahogany nuts. The two last remedies seem, in M. Bazin's hands, to exercise a real and marked influence over lupus and other scrofulides; and I have seen the mahogany oil succeed in several extremely severe and obstinate cases, that have resisted every other application.

M. Bazin advocates also creasote, nitric acid, nitrate of mercury, also perchloride of iron for Lupus vorax. But I have never seen him apply either. The iodine does not seem to be so generally successful, though it succeeds in some cases.

The ordinary application for psoriasis is tar ointment. If that produces too violent irritation, a pomade of oxide of zinc and camphor, or calomel ointments are substituted. To calm the torments of lichen and prurigo, ointments of cyanide of potassium, 5–10 centig. to 30 grms. of lard, are employed. Also ointments containing 1 grm. of calomel and 2–3 grms. of tannin to the 30 grms. Similar applications are made in chronic eczema, which is also treated by the bichloride of mercury in ointments and lotions, by M. Hardy.

For pemphigus foliacea, and cachectic or syphilitic ecthyma, much reliance is placed upon a mixture of quinquina and powder of worm-eaten wood, as a palliative.

Bazin pronounces decidedly upon the appropriateness of curing eruptive affections of children. Cazenave admits the same advisability, though he recommends precautions. Both observe that the affections left to themselves, frequently tend to become inveterate, and assume worse forms; infantile eczema degenerating into chronic lichen, benign scrofulides becoming malignant, &c. As long as the patient remains under the influence of the constitutional disease, a relapse of the affection or of its equivalent, is to be expected as a matter of course, and the physician must be prepared to combat it afresh, until the disease be exhausted. But the dangers of repercussion (upon which

Dévergie still insists), have been greatly exaggerated, and are chiefly based upon the fact, that the intercurrence of an acute disease causes the temporary cessation of the cutaneous affection, even though that be parasitic, as scabies. This (the parasitic also) returns after convalescence from the intercurrent malady. The true interpretation of the relation between the internal and external affections has, therefore, according to the St. Louis physicians, been precisely inverted. P. C. M.

PARIS, Oct., 1868.

Gonorrhœal Rheumatism.

To the Editor of the Medical Record.

SIR—According to Follet, gonorrhœal rheumatism was described for the first time by Swediaur in a medical journal published at London in 1781. It was subsequently admitted, though with some indecision, by Hunter. The French physician above-mentioned, who fully believes in the distinct existence of this form of arthritic disease, describes it as follows:

It occurs in about 1 case in 35 of urethral gonorrhœa, upon which it depends directly, as effect upon cause, and with patients who have never before suffered from rheumatism. Relapse of the urethral affection determines a similar relapse of the articular with fatal regularity. It is rare among women, probably because with them the gonorrhœal flux is more often located in the vagina than the urethra.

It declares itself at the moment that this flux is most abundant, and the latter generally diminishes with the progress of the rheumatism, but rarely ceases altogether until that has disappeared. There is, therefore, no appearance of a metastasis, properly so-called. *Début* of the arthritis is sometimes brusque —by articular pain almost always limited to one joint, especially the knee, and after that, in point of frequency, the shoulder. At other times chills, fevers, and gastric disturbance precede the arthritis, but these general symptoms are always much less pronounced than in ordinary acute rheumatism. The affection of the joints may, however, extend from the one first invaded, but the secondary inflammations are generally less intense, and often fail altogether.

Follet confirms the interesting observation already made by

Hunter, that the blood presents no inflammatory clot, and in that respect resembles the blood in chronic rheumatism. Cardiac symptoms are rare, and of little gravity. The articular pains consist at first of a sensation of stiffness and numbness, but presently become excessively severe, contusive, boring, lancinating. Swediaur describes them as *frightful.* According to Velpeau, however, they are sometimes absent altogether. The swelling of the joint is considerable; the inflammation rarely mobile, retaining its original place with great tenacity, even when it has extended its influence to other articulations.

The ordinary duration of this form of rheumatism is 6 to 8 weeks. Follet admits resolution as habitual, and ankylosis as a rare termination, but other physicians consider the frequency of ankylosis as among the most characteristic symptoms of gonorrhœal rheumatism, and Follet himself notices that the synovial is more profoundly attacked than in the ordinary disease. This termination was noticeable in the case of a young girl recently a patient in M. Gosselin's wards, and that I had an opportunity of observing. She entered the hospital for a slightly pyretic rheumatism, apparently generalized, but bearing most heavily upon the left knee. The case was at first considered an ordinary one, but in a day or two the general symptoms had disappeared, and all the articulations were disengaged, except the knee, where the intensity of the inflammation continued to increase during three or four weeks. The complete defervescence, the persistence of great pain and swelling after the redness had disappeared, caused M. Gosselin to fear the formation of a white swelling (*tumeur blanche*), although neither the patient's appearance nor antecedents indicated scrofula. The limb was placed in an immobilizing wire gutter, and the inflammation constantly combated by emollients. The inflammation finally subsided about three weeks later, but the joint was perfectly ankylosed in extension. Examination then made for the first time discovered a purulent oozing from the urethra, and the patient acknowledged the previous existence of blennorrhagic accidents.

This case is the more interesting because many persons have denied the existence of blennorrhagic rheumatism in female patients. The reason above quoted from Follet, and the greater difficulty of exploration, and more frequent attempts at conceal-

ment on the part of the women, may serve to explain this difference.

M. Fournier observes that he had only been able to find four observations of such coincidence, related by authors, of which two were by Cullerier and two by Richet, but he himself has recently come across four others all at once, and describes two at length. In the first case the pain commenced at the hip, then successively invaded the knee and the ring-finger. The wrist was somewhat swollen, but movements intact. The patient was pregnant, and examination (notwithstanding denial) discovered greenish pus at the urinary meatus. The rheumatism gradually invaded the extensor tendons of the hand, and the pain remained atrocious for three weeks. As it diminished, the gonorrhœal flux diminished also, and changed color, but the metacarpophalangeal articulation of the ring-finger ankylosed completely.

In the second case the rheumatism occupied exclusively the tendons of the pes anserinus at the right knee, and of the femoral biceps at the left. The articulations were healthy, but movement extremely painful. Complete absence of general symptoms and of rheumatismal antecedents. Coincidence of a pregnancy of several months, and of well-marked urethral gonorrhœa.

I will make a last quotation of an observation related by M. Peter, and which formed the starting-point for the long discussion at the Academy. The patient entered the hospital with a bilateral sciatica, and pain upon pressure on the spinal apophyses of the umbar and cervical vertebræ. Cutaneous sensibility was deadened in the legs, and especially the feet, which were benumbed at the soles. The gait was enfeebled and limping. Finally, the circular pain around the waist helped to decide the diagnosis of marked disease of the spinal cord.

It was subsequently discovered that the patient was also suffering from his third attack of gonorrhœa, dating from three months previous. The sciatica was of ten days' duration, and the *douleur en ceinture* three.

Three-inch scarifying cups were applied to the lumbar region, and copaiba and cubebs administered internally. The pain was immediately diminished the next morning, but the feebleness remained the same. Vapor douches were ordered after the second day. The cupping was repeated three times in the course of thirteen days, and the pain and gonorrhœa

diminished together, and markedly, on the fourteenth day, occurred a pain in the temporo-maxillary articulation, speedily relieved by laudanized cotton wool. On the 23d day, pain in the right knee, also relieved in the same manner. At this period the vapor douches were replaced by sulphurous, and these triumphed over the feebleness and numbness remaining in the limbs. A month from the date of admission the patient left entirely well, thus happily exchanging the original diagnosis for that of a gonorrhœal rheumatism. Many other similar cases have been placed on record, of which I will only mention that inserted in the Archives of Medicine by Féréol. A gonorrhœa of five months' standing was complicated by a mono-articular arthritis of the left wrist, and, a month later, by an œdematous phlegmon of the left submaxillary region, whose point of departure existed in the inflammation of a lymphatic ganglion at the angle of the jaw. In three days this phlegmon had extended from the cheek-bone to the clavicle, but without comprising the parotid. It presented a lardaceous, almost woody hardness, but was neither vasculated nor fluctuating. The redness was bright, the pain exquisite. The phlegmon was punctured with the trocar, giving issue to a quantity of pale fluid blood, mingled with serosity, and the operation was followed by complete cure.

The physicians who debate on the question actually diminish the proportion of coincidences between gonorrœha and rheumatism from 1 in 35 (as stated by Follet), to 1 in 62.

Upon these coincidences what opinion is to be formed?

In the first place, the necessity for any peculiar opinion may be altogether denied, on the ground that the coincidence is merely casual, a gonorrhœal patient catching rheumatism like any other when exposed to accidents of cold, etc. Against this idea are opposed the arguments: 1st, That in patients liable to this duplicate affection, relapse of either of its branches is almost invariably attended by the appearance of the other. Its comparative rarity therefore, should not count against its reality, more than in the case of any other disease. 2d, That the rheumatism which coincides with gonorrhœa possesses peculiar characteristics, sufficing to distinguish it essentially from ordinary articular inflammation. The first argument is sufficiently stated; the second deserves some consideration.

Reference to the description given at the beginning of this

article will show that the arthritis in question is that long known as mono-articular rheumatism, and recognized as notably different from the generalized disease, either acute or chronic. Its entire obstinacy to the quinine treatment is not the least interesting distinction that can be made in its character. The less frequent and intimate connection with cardiac disease is another important circumstance, though by no means always to be relied upon. I saw a case last summer at La Charité, of mono-arthritis of the left tibio-tarsal articulation, accompanied by a pericarditis that proved fatal. Still, in a general way, these considerations, and those already mentioned, really suffice to establish a specific separation of this disease from ordinary rheumatism. It now remains to be decided, whether this species should itself be divided into simple mono-articular rheumatism and that essentially connected, either as cause or effect, with gonorrhœa.

As I have already reached my limits, I defer this question to my next letter. P. C. M.

Association of Rheumatism and Chorea.

To the Editor of the Medical Record.

Sir—As I have had occasion already to notice, rheumatism, in the estimation of European physicians tends continually to enlarge its sphere of influence, and to take its place as a widely ramifying constitutional disease by the side of scrofula and syphilis. Under the name of arthritis, M. Bazin attacks rheumatism of the skin as endopericarditis; M. Bouilland pursues rheumatism of the heart, and Roger, Sée, Botrel, Axenfeld, Trousseau, and others, detect its malignant intervention in the production of neuroses, and especially the chorea of childhood. The connection between rheumatism and chorea had already been signalized by Stoll and Sauvages; by Bouteille, who first distinguished chorea from the epidemic St. Vitus' dance, with which it had been confounded by Sydenham; and in England Copland, Bright, Abercrombie, and Begbie, had insisted with more or less emphasis on the common parentage of the two diseases. This view was, however, deliberately defended *in extenso* for the first time, in 1850, by Botrel, in an inaugural thesis, and by Sée, in a memoir subsequently recompensed by the Academy. Trous-

seau, in his Clinique Médicale, adopts the views of Professor Sée, whom he accuses, nevertheless, of a certain exaggeration; and Roger, physician at the children's hospital, in a series of articles recently published in the Archives of Medicine, furnishes a number of observations tending to prove that in children rheumatism and chorea accompany and alternate with one another, as frequently as rheumatism and endo pericarditis in adults.

Rheumatism can no longer be defined as a mode of inflammation characterized by its predilection for serous membranes. No tissue of the economy—fibrous, muscular, mucous, nervous—is exempt from its ravages. It is pre-eminently a general, constitutional disease, whose affections or manifestations may be classed in three groups—synovial, visceral, and nervous. Neither one of these groups is a cause of the others, but all are equally rooted in the common rheumatic vice. A patient does not have endocarditis, or rheumatic pleurisy, or meningitis, or chorea, as a consequence of his articular inflammation, or as a result of its metastasis, but the internal, as well as the external affection occurs as another symptom or manifestation of the general rheumatism that has possessed itself of his entire organism. The visceral or nervous affection may precede, or accompany, or follow the articular, just as the symptoms of pain may precede, or accompany, or follow the symptom of swelling in rheumatism of the joints. But in neither case is it relative of cause and effect that exist between the elements of the double phenomena, but a common filiation in a general condition that embraces them both.

M. Sée remarks that there is hardly an affection of the nervous system that may not declare itself under the influence of rheumatism—tetanus, delirium, muscular contractions, apoplexy, meningitis, and especially chorea, which occupies us for the moment. The professor ranks in three categories the cases in which the association of rheumatism and chorea has been observed.

1st. Categ. chorea, preceded by rheumatism.....	acute artic.	41
	artic. pains.	30
2d. Chorea accompanying rheumatism.........	acute artic.	7
	artic. pains.	8
3d. Chorea accompanying external or internal rheumatism..................................	30, of which 7 were fatal	

Finally, M. Sée ranks in a fourth category a certain number of cases (17), the only ones open to doubt, where the chorea was accompanied by a visceral rheumatism exclusively. The total is then 140.

M. Roger, for his part, undertakes to furnish cases: 1st. In which the chorea develops simultaneously with the rheumatism, or so soon after the latter affection, that the common filiation is apparent at once. 2d. Where the rheumatism manifestly engenders the chorea, which in its turn occasions an attack of rheumatism, and where this alternate generation attests the parentage of the two diseases.

The first class contains three subdivisions. (A.) The chorea declares itself during the convalescence, or shortly after the presumed cure of a rheumatism. (B.) The chorea complicates the rheumatism during its period of greatest intensity. (C.) The rheumatism and chorea make their first appearance simultaneously.

The first case is the most common, the articular inflammation seeming to transform itself into chorea with most facility during its period of decline. It might be said that the general rheumatism, having expended its violence in one direction, and forced by the strength of nature or art to beat a retreat, aimed a Parthian dart as it took to flight. There is, therefore, or should be, after the apparent establishment of complete convalescence, a moment of extreme anxiety for the physician, of which the patient is happily unconscious. The fever has fallen, or even disappeared; the swelling, pain, and heat have been exorcised at the articulations; but there remains an indefinable something, sufficient to indicate that the child is still menaced by his insidious enemy. It is at this moment that the choreic movements generally make their appearance.

Obs. 1st. Polyarticular, subacute rheumatism in a child of 11 years. 15 days after recovery a severe generalized chorea. A scarlatina complicated the neurosis, and during the fever the muscular disorder was aggravated, but diminished rapidly and *pari passu* with the exanthem. An arsenical treatment, 2.10, milligrammes a day, had been instituted for the chorea, but was interrupted by the scarlatina. The heart remained unaffected.

Obs. 2d. Extremely slight attack of articular rheumatism in a child of 5 years old; a few days after recovery occurred a severe generalized chorea. During the interval, the child had been

frightened by witnessing an epileptic convulsion, but the chorea was not developed until four days later, and could not, therefore, be referred to the moral emotion, which always produces its effect immediately, when it exercises any influence at all.

Obs. 3d. The child of rheumatic parents suffers a slight attack of febrile articular rheumatism of the lower limbs, which only lasts a week. Eight days after recovery chorea commenced violently, manifesting itself by grimaces, movements of hands and feet, agitation in the gait, difficulty of speech, diminution of sensibility and intelligence. No fever, heart normal. In this case, as in many others, the intensity of the chorea was in remarkable contrast with the mildness of the rheumatism.

Obs. 4th. Acute polyarticular rheumatism, with pleurisy, in a girl of 14. Menstruation established for eleven months. Rheumatism severe, followed by a chorea of medium intensity.

Obs. 5th. Rheumatism with pericarditis in a child of 12. During the decline of the rheumatic pains, a slight attack of chorea, which disappeared in three weeks. Palpitations and rubbing sound at the præcordium persisted.

The chorea, observes M. Roger, was not dependent upon the pericarditis but upon the rheumatic vice, which, having affected two different parts of the economy, finally attacked a third.

Obs. 6th. Acute articular rheumatism; during the decline severe chorea; recovery. A year later chorea and slight endocarditis. Cure of both affections.

(b.) This Class Contains Observations of Rheumatism, Accompanied by Chorea during its period of intensity.

Obs. 7th. Endopericarditis, with such abundant effusion that the life of the patient was seriously compromised. The following year a long attack of subacute rheumatism, accompanied by a slight chorea, which disappeared gradually at the end of a month. Neither rheumatism nor chorea returned, and the heart disease was notably ameliorated.

This case is remarkable, as showing the intimate connection of the three members of the rheumatic trilogy, even when the endocarditis, developing itself first and alone, might have been considered quite independent of rheumatism.

Obs. 8th. Polyarticular rheumatism caused by cold, in a child of 14. Endocarditis and double pleurisy, nearly simul-

taneous; slight chorea the 15th day, which increases as the pleural effusion diminishes. At this moment exacerbation of rheumatic pains during several days, and as they diminish, the chorea diminishes and disappears.

In this interesting observation, M. Roger calls attention to the multiplicity of the rheumatic accidents, their succession, their so-called metastases. First, rheumatic fever, then inflammation of several joints, followed by participation of the pericardium. The cardiac phlegmasia diminishes, rheumatism resumes its ascendency, manifesting itself in a double pleurisy. This in turn yields place to the chorea, which gains ground with every inch relinquished by articular and pleural rheumatism. Finally, a month later, when the chorea begins to abate, occurs a new attack of rheumatism. The most delicate balance seemed to be maintained between the articular rheumatic phenomena, and the muscular disorders of the chorea.

(c.) Observations of Rheumatism Complicated by Chorea from the Beginning.

Obs. 9th. Several attacks of acute articular rheumatism, with endocarditis; the last complicated with a slight chorea, rapidly cured.

Obs. 10th. Two attacks of articular rheumatism at a year's interval, both complicated with chorea and cardiac affection, occurring simultaneously. Sometimes the chorea predominated, sometimes the articular rheumatism, until both affections ceased altogether, leaving an endopericarditis as a permanent heritage to the economy.

Obs. 11th. Exceedingly slight attack of rheumatism, complicated by severe chorea: pulmonary congestion, followed by endocarditis. After the amelioration of the latter affection occurs a relapse of the rheumatism, that continues to be slight. The chorea persisted in its intensity throughout the whole, accompanied by a diminution of the intelligence, but was finally cured, while the heart disease remained permanent.

(d.) Observations of Coincidence and Alternation of Rheumatism and Chorea.

Obs. 12th. Six attacks of rheumatism and five of chorea, coinciding or alternating in less than five years. Hemiplegia at

the first relapse of rheumatism, and endocarditis at the second.

The first attack of rheumatism occurred at seven years old, and was uncomplicated. Four months later, a second attack, complicated with hemiplegia and intense generalized chorea. At nine years, a third—rheumatism, this time with endocarditis, and a second severe chorea. At ten years, another combination of rheumatism and chorea. Finally at twelve, a fifth attack of chorea, this time of slight intensity. After this, the unhappy patient seemed definitely rid of his rheumatism and his chorea, but the organic heart disease persisted.

The identity between the three affections is apparent in this case: the articular inflammation, the chorea, the cardiac phlegmasia combine, replace each other, confound themselves with one another, having the same origin and the same termination, being the triple expression of a unique vice—rheumatism.

The facts proving an intimate connection between rheumatism and chorea, may be summed up as follows:

Chorea occurs frequently in children affected with rheumatism; coincidence of the two affections.

Frequently rheumatism is closely followed by chorea; *relation of cause and effect.*

Rheumatism may complicate itself with a chorea, which survives the first attack, but presently is accompanied by a relapse of rheumatism; *parentage of the two diseases.*

Rheumatism and chorea may arise simultaneously under the influence of cold, march together, the rheumatism cease, the muscular ataxy being of more chronic nature, persist, and finally, as a band of union between the two, may develop an endo- or peri-carditis; identity of nature.

Clinical experience proves, not only the frequency of the relation between chorea and rheumatism, but almost the constancy of this relation; and henceforth the description of the rheumatism of childhood should include chorea as one of its most essential elements. These ideas cannot fail to modify the prognosis both of chorea and of infantile rheumatism.

This rheumatic chorea belongs almost exclusively to childhood, and is explained by the extreme excitability of the nervous system in children, easily aroused by the influence of the rheumatic vice.

Chorea is most imminent during the decline of rheumatism, and as a complication of benign forms of the articular affection, often limited to vague, ill-defined pains, frequently called "growing pains." And there is a certain opposition between the intensity of the two affections, so that severe chorea is more likely to accompany a slight attack of rheumatism, and inversely, a severe rheumatism to be complicated by slight choreic movements. P. C. M.

PARIS, May 6th, 1869.

Letters on Albuminuria.

It occasionally happens in the history of scientific research, that the original discoverers of new truths are able to immediately divine their consequences, and foresee the problems to which they will give rise. In this case, the widest subsequent ramifications of the subject may be traced back to their germ in the propositions of the original investigator, and the various doctrines which at different periods have been professed as exclusive seem all to have been foreboded by him, and to arise merely from some unduly special emphasis which has been laid upon one or another of his words.

The comprehensive moderation with which Dr. Bright announced his discovery of renal lesions as existing in that form of dropsy which is accompanied by albuminous urine,[1] places him unequivocally among those farsighted observers who forestall the disputes of posterity by statements that embrace all sides of the questions at issue:

"Organic changes occasionally present themselves in the structure of the kidneys which, whether they are to be considered as the cause of the dropsical effusion, or as the consequence of some other disease, cannot be unimportant. I have often found the dropsy connected with the secretion of albuminous urine more or less coagulable by heat, and in these cases the liver has presented no alteration. On the other hand, in the dropsies dependent on liver disease, the kidneys have been healthy and the urine non-coagulable. Whether the morbid structure is to be considered as having, in its incipient state given rise to an alteration in the secreting power, or whether the organic change be the consequence of long-continued morbid action, may admit of doubt.

"The more probable solution appears to be that the altered action of the kidney is the result of various hurtful causes influencing it through the medium of the stomach and skin, thus deranging the healthy balance of the circula-

[1] Report of Medical Cases, vol. i, 1827.

tion or producing an inflammatory state of the kidney itself; that when this continues long, the structure of the kidney becomes permanently changed, either in accordance with the morbid action, or by a deposit which is its consequence, but has no share in that arrangement of the vessels upon which the morbid action depends."

In the first paragraph quoted, Dr. Bright distinctly disengages a triad of phenomena, of which one had been ignored entirely, another misunderstood, and the third exclusively associated with a known organic lesion. This triad, renal lesion, albuminuria, and dropsy, alone deserve the name of Bright's disease, which cannot be lawfully represented by any one of the elements taken separately. In the second paragraph he raises the important debate between the local and general origin of this disease, and the question of priority of the functional derangement or structural alterations. A collection of twenty-two cases follows, whose history and anatomical pathology embrace nearly all the forms that have since been observed. Finally, a classification of these forms, which are all admitted as equally characteristic of the disease in question, and the suggestion that these variations may possibly represent the progressive stages of an affection essentially unique.

It is certain, therefore, that whoever regards Bright's disease exclusively as a local nephritic disorder, or exclusively connected with any one renal lesion, or as an incoherent assemblage of lesions independent of one another, or as a mere functional phenomenon, not only limits the subject, but limits the intention of the observer who first introduced the subject to the medical world.

The history of the doctrines concerning albuminuria may be resumed in five periods. The first includes all time previous to the eighteenth century, during which *dropsy* was recognized, studied, treated, and even painted,[1] and attributed to a variety of causes, especially liver disease, obstruction in the veins, lesion of the lymphatics. In the middle of the eighteenth century, Cotugno discovered the second element of Bright's triad, the albuminuria. The methodical reasoning by which he arrived at his discovery is worthy of notice. He had observed that the serous effusions in dropsical patients were coagulable by heat,

[1] See the horrible picture of Gerard Dow at the Louvre—"La femme hydropique."

and ascertained that the fluid secreted by the healthy serous membranes was not coagulable. He concluded therefore that some new substance had been added to the serous secretion by the fact of the disease. This was the first step. The second consisted in an observation entirely independent of the first, namely, that when the quantity of urine passed by the patients increased, the dropsical effusion diminished. He inferred that the kidneys had exerted themselves to carry off the liquid from the serous cavities. To prove the identity between the surplus urine and the ascitic fluid, he bethought himself to search in the former for the coagulable substance which he had previously discovered in the latter. The same test produced the same result, the urine coagulated by heat, ergo it had received into its current the peccant humor of the disease, for which it constituted a valuable channel of derivation.[1]

Those who consider renal lesions as the sole and efficient cause of albuminous urine will hasten to criticize Cotugno's assumption that the albumen in the urine was derived from that in the peritoneal serum. On the other hand we shall find M. Gubler, in a recent essay,[2] citing the resorption of serous effusions as a frequent cause of albuminuria. At all events it is certain that Cotugno's ingenious reasoning led him the first (according to Rayer) to the discovery of albumen in the urine of dropsical patients.

Two-thirds of the triad were now constituted; the third link was added by Bright in his first autopsy at Guy's Hospital. The patient, John Peacock, had been suddenly attacked with dropsical swellings of the entire body, accompanied by fever and constant pain in the small of the back. The urine was sanguinolent and albuminous. Death occurred in seven weeks, and at the autopsy the liver was found to be healthy, but the surface of the kidneys completely granulated, rough, hard, and uneven. The cortical substance seemed quite disorganized, but the tubular portion was healthy.

This coincidence between dropsy, albuminuria, and renal cortical disease, was confirmed by several subsequent autopsies, and these three elements henceforth assumed definite and permanent relations with one another, in a newly organized

[1] *Cotunnius, De ischiade nervosa*, pp. 24, 25 (quoted by Rayer).

[2] *Dict. des Sciences Médicales*, 1865. Art. Albuminurie, Gubler.

disease. This is the third period, of which Cotugno's discovery constitutes the second in the history of anasarcal albuminuria.

The enthusiastic ardor with which anatomical researches were pursued in the fertile field laid open by Bright—the *éclat* of the school of Rayer and of the lessons of Martin Solon—the application of the microscope by Valentin[1]—all tended to concentrate attention upon the structural alterations of the kidney, as the sole and essential cause both of albuminuria and of the anasarca which frequently accompanies it. All the anatomists confirmed Bright's original statement—that a multiplicity of lesions were discoverable coinciding with albuminuria. The opinions differed, however, concerning the mutual relations of these lesions—the German school, after Frerichs, deciding that they represented progressive stages of a unique morbid process; the English, nearly unanimous in maintaining their independence, or in selecting one or another among the forms as alone characteristic of Bright's disease. This is the fourth period.

Finally arrives the fifth, which has by no means supplanted the fourth, but coincides with it, being rather a mode than a period of thought Here the investigator refuses to arrest his researches at the kidneys, as the essential and sufficient agent of albuminuria, but seeks, in the organism at large, the common cause of their lesion, and of the passage of albumen in the urine. Valentin himself had suggested this extensive search, in the very passage in which he describes the first microscopical examination of a diseased kidney. "The kidneys are only the receptacle of the abnormal urine," he writes, "and the real disorder of secretion must be sought further removed, and in the blood."

A multitude of questions of the highest interest are resumed in these two groups of pathogenetic theories. To commence with the first group, we will first describe in simple succession the different alterations that have been observed in the structure of the kidney; afterwards the classifications that have been framed of these lesions, from the Report of Bright in 1827, to the recent thesis of Cornil,[2] in 1869; finally the inferences that may be drawn from purely anatomical investigations; clinical histories,

[1] *Repertorium für Anatomie et Physiol.* 1837.

[2] *Cornil, Nephrites albumineuses*—Thèse de concours pour l'agrégation, 1869.

and their combination with the facts of pathological anatomy, association and causes of symptoms coincident with albuminuria, prognosis, general pathogeny of this phenomenon, and the diseases in which it occurs, with their treatment, &c. These topics will form the subject of other letters.

The cases described by Bright may be resumed, as he in fact resumes them, into five forms, of which three are principal, and the other two barely mentioned. In the first case "a state of degeneracy exists, which seems to mark little more than a *simple debility*." It is not very clear what is meant by this "debility of the kidney," but the description given by Bright is readily recognizable, and perhaps covers several distinct forms that later are separated by the microscope. The kidney loses its firmness, and becomes pale, more or less mottled with yellow, externally and internally (in the cortical substance). In what appears to be a more advanced stage of the same lesion, are found on the surface, white portions, somewhat raised above the surface, and upon which ramify starlike vessels. Considerable spaces (the same?) are quite impermeable to injections. The tunic adheres closely. The cortex is a uniform yellow color, sprinkled with small, opaque, and indistinct yellow spots. The size of the kidneys is not altered.

This form, says Bright, may be observed in cachexias, even unaccompanied by dropsy, as in phthisis, diarrhœa, and ovarian tumor; urine only slightly coagulable. This seems to correspond to the third form described by Rayer.[1] The cortical substance is smooth and pretty equally colored throughout, being pale yellow, or very slightly rose-hued; sometimes the tint is so remarkably pale as to resemble an eel's skin. Here and there appear points of injected vessels, or brown and slate-colored spots seeming to originate in some former sanguinolent effusion.

At an autopsy performed at La Pitié yesterday, remarkable on many accounts, occurred a fine example of this anæmic kidney. The patient had succumbed to repeated attacks of hæmatemesis, dependent (as was only clearly proved by the autopsy) upon simultaneous cirrhosis of the liver and spleen.[2] The kidneys were

[1] *Traite des maladies des reins*, 1840.

[2] The latter organ was 24 centimetres long, and 12 broad, descending only two finger-breadths below the false ribs, but pushing up the diaphragm as high as the 5th rib and touching the liver behind the stomach.

normal in size, flattened, and rather less firm than usual. The capsule was removed with remarkable facility, and the surface of the organ appeared pale, smooth and polished as marble—white, slightly tinged with rose lilac. Here and there appeared little scarlet stellated vessels, isolated, or united in small groups. On section, the same smoothness and uniformity of color were observed throughout the two substances. Their relations to each other seemed normal, unless it were that the cortical substances were slightly diminished in diameter.

But besides these unequivocally anæmic kidneys, are others whose pallor is explained by a commencement of fatty degeneration. Martin Solon[1] describes as the initial stage of the "third degree," a condition where the kidneys are but slightly hypertrophied, with surface smooth and polished, and an extremely pale yellow hue, like that of the pancreas. Johnson[2] repeatedly describes the "smooth, mottled,—or waxy uniform yellowish white kidney," as in a state of fatty degeneration. Cornil[3] observes that in temporary albuminuria, the kidneys, to the naked eye, only differ from the normal appearance by a grayish[4] color, and a certain opacity of their cortical substance; but on microscopical examination with low magnifying power, 40–50 diameters, the convoluted tubes are found sombre and opaque to transmitted light, and filled with tumefied epithelial cells. These are infiltrated with proteic and fatty granulations, and the infiltration is the cause of the opacity and whitish appearance of the cortical tubes.

There is a third morbid process which may be indicated by these appearances described by Bright and Rayer. In the very beginning of the *amyloid degeneration* the kidneys often retain their normal size, the capsule is easily removed, the surface is extremely polished, and its coloration, as also that of the interior, pale, anæmic, slightly yellow in the cortical substance. All parts of the renal parenchyma affected with amyloid infiltration are impermeable to injections, which therefore dot the surface of the kidney with red spots and streaks, contrasting with the pale ground. Another important character is the occurrence of the

[1] *De l'Album inurie*, 1838. [2] *Med. Chir. Transactions*, 1846–1859.

[3] Loc. cit.

[4] "Coloration grise,"—almost the same as our pale yellow, or yellowish white.

amyloid affection in the course of exhausting cachexias, especially dependent upon phthisis and osseous suppuration. Only a small quantity of albumen ordinarily exists in the urine.[1] It is remarkable that Bright signalizes all three of these circumstances in his Case III., whose autopsy furnishes him with the type of his first form of renal lesion.

Although in some cases the amyloid degeneration may be mistaken for anæmia, or even a healthy condition of the kidneys, it is generally distinguishable, even in the incipient stages, by the hardness and leathery consistence of the kidney, and by the hypertrophy of the cortical substance. Microscopical examination, as we shall presently see, easily completes the diagnosis. *En résumé*, however, in the *slightest* (we do not say the *initial*) form of change recognizable in the kidney by the naked eye, the organ is smooth, polished, pale, uniformly colored in subdued opaque tints that vary between grayish white and faint yellow, having lost its natural rosy hue, and something of its natural consistency, but having retained its volume and the normal relation of its two substances to one another. Three different morbid processes may be indicated by these appearances—anæmia, a commencement of cellular infiltration and fatty degeneration, the initial stage of the amyloid affection.

In the second form described by Bright,

"the whole cortical substance of the kidney is converted into a granulated texture, with copious interstitial deposit of an opaque white substance. As the disease progresses, this deposit becomes more abundant, and innumerable specks are strewed through the kidney. Finally, granulations become visible externally in numerous slight uneven projections on the surface of the kidney, which is more or less enlarged. The whole cortical structure is often converted into a yellow substance like fat."

This corresponds to the fourth form of Rayer, who repeats Bright's description. He observes further, that the granulations vary in color from flaky white to yellow, are the size of a small pin's head, or drawn out into lines resembling flakes of curd, which seem to continue with the streaks in the cones. They are all veiled by an extremely delicate lamina which covers them like a varnish. The surface of the kidney over which they are strewed is perfectly smooth: the cortical substance, in which they also appear, is hypertrophied and projects between the cones,

[1] Jaccond, *Clinique Médicale*, 1867.

whose volume is normal. The hypertrophy of the kidney is therefore exclusively at the expense of its cortex.

Martin Solon describes these granulations (which he considers as a comparatively rare form of lesion) as "white, creamy, pultaceous, seeming to depend rather upon a sort of interstitial exhalation than a degeneration of tissue." He places them in his fourth form of albuminuria.

Frerichs[1] includes the granulations in his second or exudative stage of Bright's disease. According to him, the surface of the kidney is still polished between the granulations, and the capsule, though thickened, is easily removed. But Cornil, who seems to associate a certain degree of Bright's third form (hard solutions) with the granulations, declares that the surface is rough and unequal, and that in stripping the capsule it is difficult to avoid removing slices of cortical substance.

Johnson[2] establishes the granulations in a third form of lesion, which itself is only a slight modification of the second—the large, pale, anæmic, wax-like (or fatty) kidney without granulations. In addition to the granulations, numerous red spots dot the external and anterior surface.

Christian[3] admits the granulations as the second of two principal forms, inflammation and morbid degeneration. Jaccond follows Frerichs implicitly.

Andral, in 1823,[4] seems to have anticipated Bright, in a description of this form of the affection. At the autopsy of a young girl, who had succumbed to dropsy, the kidneys alone were found to be morbidly altered. "The cortical substance and a part of the tubular, were constituted by *whitish granular tissue*, divided in little masses which were separated by naturally colored reddish parenchyma."

We may justly approximate to the completely granular kidneys the second form described by Rayer, in which the kidneys are enlarged, softened, the cortical substance entirely yellow, and the surface offering a remarkable mixture of hyperæmia and anæmia. The second and the advanced degree of the third form, established by Martin Solon, exactly correspond to these subdivisions signalized by Rayer and Johnson.

[1] *Die Brightische Nierenkrankheiten und deren Behandlung*, 1851.

[2] Loc. cit. Also *Med. Times & Gazette*, 1858.

[3] *Monthly Journal*. 1851.

[4] *Clinique Médicale*.

The close connection that exists between the large, smooth, yellow non-granular form, and the large yellow granulated variety of renal alteration, is rendered evident by microscopical examination.

Valentin,[1] in his first investigations, announced that the granulations were formed by masses of convoluted tubes distended by yellowish gray material, and hence more distinctly visible than usual. The straight canals were empty, or contained a small quantity of fluid. The Malpighian corpuscles were unaltered. Frerichs, after noticing the mixture of hyperæmia and anæmia indicated by the alternation of red and yellowish white on the surface of the kidney, also describes the convoluted tubes as distended by an exudated material, chiefly contained in the epithelial cells, and composed of fatty and proteic granulations. The cells lose their polyhedric form, become round, then irregular, and finally crumble away into a confused detritus, which blocks up the tubes and renders them opaque.

A finer exudation is formed in the Malpighian capsule, around the vascular tuft, as soon as the obstruction of the convoluted tubes begins to seriously interfere with the current of urine. This material, consisting of fibrine mixed with fat globules, covers the capillary glomerulus with thick layers, interposed between it and the capsule, and their pressure, counterbalancing that of the blood in the interior of the vessels, gradually arrests the transudation of water. The blood often flows back, therefore, from the corpuscle where its presence has become useless, and the capillaries are left empty.

Associated with the granular and fatty exudation is another of pure fibrine, which forms hyaline cylinders,[2] that assume the form, shape, and size of the convoluted tubes. When formed in tubes whose epithelial cells are falling from their walls, they are "granulated," because the desquamated epithelium becomes embedded in their substance. But the cylinders, exuded into

[1] Loc. cit.

[2] According to Frerichs, these hyaline cylinders are formed by a simple process of inflammatory exudation, and identical with that which determines the passage of albumen in the urine. But Cornil considers them due to a colloid secretion from the cells, or a colloid transformation of the cells, comparable to that which takes place in the colloid degeneration of the cells of the thyroid body.

tubes entirely stripped of epithelium, are large, pale, and waxy, and perfectly smooth.

Transverse section of the distended tubes often offers the appearance of little cysts, disseminated over the kidney. These cysts had been noticed by other observers,[1] but their nature had generally been misunderstood. Cornil affirms with Valentin that the granulations of Bright are formed by distended convoluted tubes.[2] When the exudation consists of epithelial cells, simply desquamated, or filled with proteic (fibrinous) granulations, the granulations of Bright are grayish, opaque, or even demi-transparent. When the granulations of Bright contain fat, they become decidedly yellow in color. The uriniferous tubes and glomeruli in the neighborhood are normal or atrophied, and their collapse contributes to render the distended convolutions more prominent. Ecchymoses may occur near the circumference and they result from rupture of the capillary vessels submitted to excessive pressure by the obstruction to the circulation caused by the exuded deposit. The red points, once supposed to be distended malpighian corpuscles, result from such a rupture into the extremity of a uriniferous tube. The glomeruli, as we have seen, are empty, or distended by fibrinous exudation—not blood.

The pressure of the granulations known as "Granulations of Bright" (and which must be carefully distinguished from others, more commonly met with, and which will be described further on) constitute, therefore, but a minor detail in the morbid alteration in question. The essential circumstance is the degeneration of the epithelium in the convoluted tubes. If this degeneration is generally diffused, so that the cortex is uniformly distended, the surface of the kidney, both external and on section, remains smooth and uniform. If the alteration is unequally distributed, certain bundles of tubes will be distended, at the same time that others are normal or collapsed, and will consequently become prominent above the level of their neighbors.

The hypertrophy of the kidney, exclusively at the expense of its cortical substance, its coloration, anæmia and hyperæmia, and its diminution of density, are all explained by this engorgement of

[1] Wilkes, *Guy's Hospital Reports*, 1852.

[2] Wilkes (loc. cit.) considers the granulations to be sometimes formed by the flaky deposit, scattered over the surface—the yellowish specks described by Bright and Martin Solon.

the cortical tubes with epithelial cells, whose abnormal proliferation has been followed by troubled tumefaction, and finally by desquamation. But it will be noticed that under the general description of the minute anatomy of the "large, white kidney" have been united three very different conditions. In the first, epithelial cells are filled by proteic granulations. Second, the same are distended with fat globules. Third, the uriniferous tubes are stripped bare, and collapse when their contents have been swept away by the stream of urine which continues to pass through them more or less freely.

The relations of these different conditions to one another will be examined later. At present it is only necessary to signalize their existence, and to decompose the second as we have previously decomposed the first form of alteration described by Bright, into three distinct lesions.

Besides the fatty degeneration that accompanies and partially determines the "granulations of Bright," Cornil admits into the group of "*nephrites albumineuses*," the stearosis occasioned by poisoning with phosphorus. Lebert[1] and Rannier[2] relate several cases of this lesion, whose dependence upon phosphorus was first insisted upon by Von Hauff in 1860,[3] though in 1859 Rokitansky had already related three cases of stearosis of the liver and kidneys, that he attributed to phosphoric poisoning.

In the cellular tissue which surrounds the kidneys (remarks Lebert) often exist little ecchymoses, also noticeable on the mucous membrane of the basin and calices. Capsule is smooth, easy to separate. On the surface of the kidney appears the mixture of hyperæmia and anæmia, already described by Rayer in the "large yellow kidneys," and dependent upon the obliteration of a part of the blood-vessels by the exudation, and the engorgement of another part in consequence of this obstruction. Volume of the kidney normal or increased. Fatty decoloration unequally distributed. Cortical substance yellow, and often notably atrophied. Surface smooth and shining, and of pasty consistency.

On microscopical examination the convoluted tubes are found to be engorged with fat granulations, which cease suddenly on the threshold of the glomeruli. These latter are congested, but otherwise perfectly healthy. According to Cornil, the fatty

[1] *Archives de Médicine*, Sept. 1868. [2] *Archives de Médicine*, 1863.
[3] *Würtemberger Correspondenzblatt.*

infiltration extends into the straight tubes, affecting especially the loop tubes of Henle, which become entirely black.

The fibrinous cylinders appear in the urine like a black, finely granulated, cylindric mass, filled solid, and thickly studded with fat granulations. This fatty degeneration is only observed if death be postponed beyond the third or fourth day. It is accompanied by similar lesions in the liver and heart, a circumstance unfavorable to the supposition that the renal stearosis is an essential affection of the secreting organ of the kidney. The rapid participation of the straight tubes, long intact in Bright's disease, and the scanty or doubtful amount of albumen that generally exists in the urine, also militate against the justness of Cornil's classification. But the question deserves more elaborate consideration.

We arrive at the third and last form described by Bright. "The kidney is rough and scabrous, lobulated, and rising in numerous small eminences. The feel is hard, like that of cartilage. The tubular portions *are drawn near the surface:* there seems to be contraction of every part of the organ, with less interstitial deposit than in the preceding variety."

It is this form which is often erroneously called "the granulated kidney." The true granulations of Bright are, as we have seen, extremely small, mere specks—soft, yellow, and seeming to be exterior to the parenchyma. The false granulations (which in reality better deserve the name) are larger, the size of a hemp seed, hard, evidently formed by the inequalities of the parenchyma itself, closely adherent to the capsule. The kidney is small, "contracted in every part," and has become famous as "the small, hard, contracted kidney," recognized and described by all authors. By the picturesque expression that "the tubular portions seem drawn near the surface" (appearance admirably represented in his plates) Bright indicates the extreme atrophy of the cortical substance, which he does not appear to recognize as the most special feature of the lesion.

Rayer repeats Bright's description in his sixth form. Martin Solon mentions it as "induration with atrophy," in a class consecrated to "accidental degenerations." Wilkes describes the "puckered uneven" surface of the kidney in this form, and the complete wasting of the cortical substance, by which the kidney shrinks to ½ or ⅓ its normal size. As a rule, there is no pul-

taceous deposit, but a large addition of fibrous tissue. Cornil remarks the multiplicity of lesions which may be found in this atrophic form—transparent granulations, calcareous incrustations of the glomeruli, atheromatous alterations of the vessels, cysts formed by distension of the tubes, and great abundance of fibrous tissue, as in the interstitial nephritis that occurs independent of Bright's disease. P. C. M.

PARIS, July 1869.

To the Editor of the Medical Record.

SIR—The microscopic descriptions given by Frerichs and Wilkes explain the appearances in the atrophic form. The uriniferous tubes, stripped of their epithelium, universally collapse, as they had already begun to do in the granulations. (See last letter.) The straight tubes also become indistinct, and at last almost undistinguishable, until separated by fine needles. They are stifled in connective tissue, which originates both in the proliferation of that normally existing between the tubes, and the organization of the fibrinous material exuded from the vascular plexus which surrounds them. The malpighian corpuscles in many places are wasted to half their usual size, having been destroyed in the crumbling away of the proteic and fatty granulations in the interior of their capsule. Others distend into little cysts, by the accumulation of urine, when the convoluted tube is so blocked up as to oppose its passage, at a moment when the malpighian tuft, still unaltered, continues its watery secretion. Others, again, remain distended by solid exudation, and rise in eminences above the uriniferous tubes strangled in connective tissue, thus forming the hard granulations charactertistic of the atrophic form. They differ from the granulations of Bright simply in the greater condensation of their contents. This is the stage of *atrophy* described by Frerichs as the culmination of the disease.

Under the first anatomical form described by Bright, we have considered ourselves justified in recognizing, among other lesions, an incipient degree of an alteration that has been described as the "Amyloid Degeneration." In the complete development of this form of renal affection the kidney is voluminous, heavy, and pale, and at first sight might be mistaken for the ordinary "large white

kidney," with which, no doubt, it has often been confounded.[1] Closer inspection, however, shows that its consistency is remarkably firm, hard, and tough, even leathery. The surface is uniform and smooth, without any mottling or deposit, except in the case of concomitant fatty degeneration of some tubuli; the cortical substance is hypertrophied, and the whole mass of the kidney appears composed of one uniform albuminous semi-translucent substance, except at the apices of the cones. This appearance is, however, only characteristic of extreme cases. In others,[2] the cortical substance is pale yellowish white, the convoluted tubes opaque, with little dots and streaks, and the straight tubes alone semi-translucent. In some cases (Harris) the malpighian corpuscles are scarcely apparent. In others (Jaccoud[3]) they appear upon the surface of section, white and transparent, like brilliant drops of dew (Meckel).

In the autopsy performed by Harris, microscopical examination of the fluid expressed from the kidneys, discovered blood cells, epithelium from straight tubes, cells filled with minute oil globules, and granular and fatty detritus of cells and nuclei. Those portions of the cortex which had appeared opaque to the naked eye, now showed as a black deposit contained within the tubes, and were seen to consist of granular matter studded with oil globules. The malpighian corpuscles were remarkably distinct and semi-transparent.

The pathognomonie appearance of the amyloid kidney is only observed after the addition of iodine and sulphuric acid. A drop of a solution of iodated iodide of potassium, added carefully to the slice under the microscope, colors the malpighian corpuscles a transparent carmine by transmitted light, orange by reflected; streaks of the same color diverge from the corpuscle, following the direction of the afferent arteries of the tuft. The same reagent colors orange red the black opaque deposit that surrounds and invades the convoluted and straight tubes. On the addition of a drop of sulphuric acid, the color changes to dark purple, blue, and finally, after a quarter of an hour, deep red, brown and black.

The seat of this deposit is principally in the coats of the

[1] Wilkes' *Lardaceous Diseases, Guy's Hospital Reports*, 1856.

[2] Harris, *Lancet*, 1859.

[3] *Clinique Médicale.*

arterioles and arteries, beginning with the former.[1] According to Virchow the muscular coat is attacked first. Each cell fibre is replaced by a compact homogeneous substance, in the middle of which may be at first observed a central space corresponding to the nucleus which has disappeared. Gradually, however, all cell structure is lost, and there remains only a fusiform mass, in the midst of which it is impossible to recognize either membrane, or nucleus, or contents. When the muscular coat is completely invaded, the inner and outer tunics became involved, and ultimately the deposit extends to the entire parenchyma nourished by the arteries. This parenchyma becomes ischæmic from a double cause. The small arterioles lose their propulsive contracting power by the destruction, the sort of petrifaction of their muscular coat; and afterwards the thickening of the three tunics diminishes, and even effaces the cavity of their canal.

According to Grainger Stewart the hyaline cylinders assume the characteristic coloration with iodine. But Cornil observes that they undergo no further change upon the addition of sulphuric acid, and considers their coloration to result from simple imbibition of the iodine. This latter author has always found the deposit extended to the epithelial cells, in cases where albuminuria existed.

There is still another and highly important form of renal lesion, which Bright describes, but without assigning it a place in his classification. In his case 14, where the anasarca was acute and the urine smoky and sanguinolent as well as albuminous, the autopsy discovered the kidneys in a different condition from any of the others previously examined. They were large, less firm, without adherence to the capsule, and of the darkest chocolate color, tinged with a few white points and a great number of black, so as to look like fine-grained porphyry. This color pervaded the entire organ, but the striations of cortical and medullary substance were preserved. A considerable quantity of blood oozed from the kidneys upon pressure.

This description applies to an intense hyperæmia of the kidney, and is repeated by Rayer, Martin Solon, and Frerichs, in their first form or degree of Bright's disease. According to the latter, the volume of the kidney may be nearly doubled, and that principally at the expense of the cortical substance, which is dark

[1] Virchow, *Pathologie Cellulaire. Trad. française.*

red and friable. The renous plexuses on the surface of the cortex, and surrounding the vessels of the pyramids, are distended with blood, and the mucous membrane of the basin covered with vascular ramifications. The malpighian corpuscles are red, distended, and more distinct than usual, and capillary apoplexies are frequent, either in their interior or in their neighborhood.

The epithelium is not much altered in this form, but the canals are filled with coagulated fibrine, in the form of white, transparent cylinders. These sometimes are observed projecting from the uriniferous tubes, sure proof that they are formed in their interior.

In the midst of this general congestion, appear grayish bands in the cortical substance, formed by the pyramids of Ferrein, and opaque white lines in the pyramids of Malpighi, consisting of the tubes of Henle.

The foregoing descriptions embrace all the alterations of the kidney which have been observed in connection with dropsy or albuminous urine, in the triad constituting Bright's disease. It will be seen that they all involve some alteration of the malpighian corpuscles, or the interior of the uriniferous tubes, and justify therefore the general name of "parenchymatous lesions," which has been applied to them. The "interstitial nephritis," which affects the connective tissue between the tubes, with its various forms, simple nephritis, metastatic nephritis, and chronic nephritis or cirrhosis, and the special varieties signalized by Rayer, toxicologic and arthritic[1] nephritis, these should be most appropriately noticed during the discussion of the causal relations that unite albuminuria to the parenchymatous affection. For, by the absence of this phenomenon in the case of purely interstitial affections, we are at once provided with a logical dilemma that clears half the field open to hypothesis, and forcibly limits investigation to the other half.

I have enumerated the lesions of Bright's disease in simple succession, precisely as they might present themselves to any one in a series of autopsies. The relations between these lesions, their arrangement in separate forms or progressive stages, the efficient and remote causes of hyperæmia, granulations, fatty degeneration, atrophy, amyloid infiltration; the nature of these

[1] See also Todd, *Clinical Diseases of the Urinary Organs*, 1757 and Ball, *Visceral Rheumatism*, *Thèse de Concours*, 1865.

various morbid processes, their possible influence upon the character of the urine and on the production of anasarca—all these questions, discussed at first, exclusively with the anatomical elements collected in the present letter, will form the subject of the next. P. C. M.

To the Editor of the Medical Record.

SIR—In a preceding letter we have described the renal lesions of Bright's disease, *per enumerationem simplicem.* It is now necessary to inquire into their logical relations to one another. The various conditions known as congestion—exudative nephritis, acute inflammatory nephritis, catarrhal nephritis, tubular nephritis, desquamative nephritis—fatty degeneration, smooth large mottled kidney, fatty stage of inflammation, interstitial nephritis, gouty kidney, atrophy, small contracted kidney, granular kidney, cirrhosis—finally, amyloid degeneration, may be regarded in the light of one of two hypotheses, each equally famous. They may be supposed to belong to diseases, as distinct and independent as pneumonia and phthisis, linked together by the common symptom of albuminuria, as these by the common symptom of cough. Or, on the contrary, they may be regarded as successive steps in the evolution of an identical process, as are the gray miliary granulation, the cheesy pneumonia—the softening and excavations in the evolution of pulmonary tuberculosis. The practical importance of decision between these hypotheses is no less evident than the speculative interest. The boundaries between the two great divisions into acute and chronic disease most urgently require settlement. If the renal lesions characteristic of long standing albuminuria invariably commence in organic degeneration, the disease is at once chronic and incurable from the outset. On the other hand, the acute dropsy after scarlatina and exposure to cold, is radically different from Bright's disease, and can have no more tendency to pass into it than endocarditis into fatty degeneration of the heart. But if the initial stage of certain chronic affections be identical with that of acute affections tending spontaneously to recovery, the hope may be entertained of arresting them also in their march, if taken in time. Finally, the obstinacy of certain other forms of disease may be explained and predicted by considerations drawn

from the differences they present in the initial lesion, differences which indicate their radical independence.

Again, the value of any one symptom among the vast array coincident with albuminuria cannot be determined until we know whether it is necessarily connected with the whole range of Bright's disease, or belongs to a group standing apart by itself. Does abundance of albumen in the urine threaten uræmia as well as dropsy? Is diarrhœa likely to supervene when the heart is affected? Does hæmaturia indicate an exacerbation of the disease, and the disappearance of albuminuria its cure? Will a patient live longer whose complexion is white or sallow? Does lead or alcohol tend more fatally to the production of Bright's disease? If dropsy and albuminuria be the pathological signs of renal disease, how explain their absence notwithstanding the existence of extensive structural alterations of the kidney?

These questions, suggested at random, indicate the infinite confusion that results, in the absence of the elementary analysis that shall rigorously attach each physical sign to a definite lesion of the kidney, and, grouping symptoms rationally or empirically around such elementary lesions, anticipate their association in the same manner as these are known to be associated.

We have shown that all alterations of structure in the kidney may be referred to one of three heads, according as they affect the blood-vessels, the uriniferous glands and tubes, or the intertubular connective tissue. In the list mentioned at the beginning of this letter, the first and last class of alterations (congestion and amyloid degeneration) affect the vascular structure; the second and third class (exudative nephritis, &c., fatty degeneration, &c.) involve the glandular elements; finally, the fourth class (interstitial nephritis, &c.) attack the connective tissue. Two questions are, therefore, involved in the decision of the theory of successive stages. First, Can an alteration affecting one element of the kidney pass into another confined to the same element? Second, Can such an alteration pass to another element, or to another alteration affecting another element? and having ascertained the possibility of such progression, we must further inquire into its necessity or invariability. Reply to these inquiries demands, first, study of the combinations that may be observed after death, in the same kidney, of the lesions characteristic of each form of Bright's disease. Second, study of the combination

and order of succession that may be presented during life by the groups of symptoms characteristic of the same forms.

We have already described at length the anatomical lesions. Before studying the combination of signs and symptoms, it is necessary to define the groups, and ascertain if they can be rationally or empirically attached to special lesions.

And here it is necessary to distinguish. Zimmerman,[1] in despair at the variety of symptoms that may coincide with the same anatomical alterations, seeks in the blood and general state of the system an explanation that pathological anatomy alone is unable to give. This confusion mainly arises from the qualitative differences that may be introduced by qualitative variations. Thus, to anticipate in our description, with equal degrees of alteration of the glandular and interstitial elements of the kidney, uræmia would be imminent in the first case, and scarcely possible in the second. But should the interstitial tissue become still more compromised, uræmia might supervene with the same facility as in the course of tubular disease, and the same result be reached later and by a somewhat different mechanism. Time must, therefore, always be taken into account, in determining the relation between a symptom and any given lesion.

The composition of the urine affords the best point of departure, especially in regard to its water, urea, and the morphological elements that may be abnormally present. The conditions which determine the transudation of albumen, being themselves open to much discussion, the variations of this constituent are of less value in the elementary diagnosis of renal lesion, and will be considered later. But in the absence of general causes, such as exist in cholera or fevers, the diminution of the quantity of water excreted by the kidney necessarily implies either alteration of the capillaries through which it should have been transuded, or obstruction of the tube through which it should have been excreted. Again, the presence of blood in the urine is positive proof that the capillaries have been ruptured by over-distension; of epithelium, that the urinary tubes are losing their lining; of oil globules, that fat exists in abnormal quantity in the gland cells; of casts, that some foreign substance has exuded into the tubes and moulded itself upon them. If the diameter of these casts be

[1] *Deutsche Klinik*, 1855.

less than that of the tubes lined with epithelium ($\frac{1}{1000}$) it is certain that the epithelium is still in place; if large ($\frac{1}{500}$) that the tube is denuded. Casts covered with epithelium indicate desquamation of the urinary tubes; granular casts, the crumbling away of their epithelium, after a longer duration of disease; casts black and shining with oil globules, extensive fatty infiltration and degeneration, with complete destruction of epithelium. The qualitative analysis of these elements must be controlled by estimate of their relative abundance. In small quantity they have little significance, since the fall and renewal of epithelium cells, the deposit of fat between their wall and nucleus, the exudation of pale, transparent, finely granular cylinders formed of mucine (Cornil) are ordinary phenomena of health. Moreover, Johnson and Dickinson[1] have noticed an abundant deposit of oil in the renal epithelium, in cases of chronic extra-renal disease,—deposit formed here, as in the epithelium all over the body, to remain temporarily and then be absorbed. On the other hand, an immense amount of oil, associated with complete destruction of the epithelium, is as unsafe an indication of Bright's disease as a small quantity contained in normal cells, for it is characteristic of the acute, fatty degeneration caused by poisoning with phosphorus,[2] and is entirely independent of idiopathic nephritis. Finally, a considerable amount of urinary deposit may be formed in consequence of lesions occupying a comparatively insignificant extent of the renal structure. The inferences drawn from it, therefore, should always be controlled by considerations of the quantity of urine and of urea, state of the blood, general symptoms, &c.

Congestion.—The characteristic sign of congestion is the presence of blood in the urine,—and reciprocally, we have seen that the presence of blood always indicates some degree of congestion. It may exist in microscopic quantity, only recognizable by the form of the blood-corpuscles or be sufficient to render the urine smoky or black. Sanguinolent urine is necessarily albuminous, even in the absence of conditions that might determine the transudation of albumen through unruptured capillaries. In this case the albumen is derived from the effused blood, and varies in

[1] *Pathology of Albuminuria.*

[2] Cornil, *Thèse de Concours*, 1868. Ranvier, *Archives gen.*, 1863. Lehbert, *Archives gen.*, 1865.

the same proportion, which is not the case when congestion complicates pre-established albuminuria. There are two conditions in which the kidney becomes entirely congested, 1st, during the convalescence from certain acute diseases,—principally scarlatina; 2d, idiopathically, after exposure to cold.

In the first case, the urine becomes suddenly albuminous and smoky, and the cellular tissue throughout the body invaded by œdema, but there is no pain in the loins, fever, or any exacerbation of the original disease. In the second case, when the affection occurs in the midst of health, the general symptoms are more distinctly marked; there is general malaise, lassitude, perhaps slight fever, loss of appetite, nausea. An attack of acute congestion, however, remains scarcely ever limited to the repletion of the blood-vessels. The lining of the uriniferous tubes is irritated, and the congestion passes insensibly into the second form—catarrhal nephritis.

Its characteristic sign is the presence of epithelial cells in the albuminous urine, mingled with the blood-corpuscles. These are remarkably abundant in scarlatinous albuminuria, and when death occurs in the course of this affection, the cortical substance is found nearly white, all the convoluted tubes being stuffed with young epithelium. According to Rindsfleisch, these young cells come from cells formed in the conjunctive tissue of the peripheric stroma, passing into the uriniferous tubes across pores pierced in their basement membrane. In the urine, as in the tubes, they are found voluminous, and in a state of troubled tumefaction distended with albuminous granulations that partially mask the nucleus. These disappear on the addition of acetic acid. Many of the cells are found in various degrees of disintegration, or even filled with fat globules, which appear as soon as the desquamation has lasted for a few days.

This simple catarrh, the almost immediate consequence of congestion (which itself may be insufficient to render the urine smoky, and only be manifested by the presence of blood-corpuscles in the urine), is not attended with symptoms of greater severity than the congestion alone. In scarlatinous albuminuria the renal lesion often arrests itself at this point. In the albuminuria supervening after exposure to cold, the affection generally progresses to a third stage, Exudative Nephritis.

The transition is marked by the appearance in the urine

of still another morphological element—casts or cylinders of various forms.

We have seen that certain extremely pale, finely granular cylinders, about equal in diameter to the cavity of the uriniferous tubes, still invested with epithelium, may be observed in the urine in health, and are supposed by Robin to be formed of mucine. But the hyaline casts are quite transparent, and with firmly defined outline. They are of small diameter, and mingled with casts covered with epithelial cells, and others whose surface is moderately granular, attesting the crumbling away of the disintegrated epithelium. These casts may be present at the first examination of the urine, the affection having begun with greater intensity, and advanced rapidly to the exudative stage. In this case the illness has been ushered in with chills and shivering, not very intense, but general and prolonged. The chills are followed by fever, pain in the lumbar region of the back, vomiting, violent headache. Drowsiness is a frequent and characteristic symptom, of great value in distinguishing between the acute nephritis and other febrile affections, especially small-pox at the initial stage, where pain in the loins is a symptom even more noticeable than when the kidneys are the seat of disorder. The urine diminishes notably in quantity, and at the beginning its specific gravity may even be increased, as in other phlegmasias and fevers. The reduction of water is more rapid than the reduction of urea, so that the proportion of solids may seem relatively increased, although they are absolutely diminished. In a few days, however, the impairment in the cell functions becomes manifest, and the specific gravity of the urine falls from diminution in the elimination of its solid materials. Micturition is frequent, owing to reflex irritation of the bladder. The urine contains blood in various proportions, as previously noticed, and is strongly albuminous—sometimes becoming nearly solid on the addition of nitric acid. Œdema is rapidly formed and extended, constituting the most conspicuous phenomenon of the group, and which has procured for it the title of acute dropsy. Interpretation of the real significance of the casts found in the urine is of considerable importance, since upon it has been made to depend the theory of the renal lesion of which they are symptomatic. According to Frerichs, these casts consist of fibrine, exuded from the blood-vessels under circumstances analogous to those of any

parenchymatous inflammation, and coagulated in the cavity of the uriniferous tubes, upon whose form they are moulded. Frerichs designates the lesion, therefore, as marking the exudative period in albuminous nephritis, and Virchow calls it Croupal[1] Nephritis, the analogue of pneumonia.

Grainger, Stewart, and Dickinson follow Frerichs in this interpretation, and Traube observes that the variations of pressure in the blood-vessels constitute a regular scale, of which the first degree determines the transudation of albumen, the second fibrine, and the third ruptures them, causing the effusion of the blood itself. In hæmaturia, without casts, it is the capillaries in the Malpighian plexus that are ruptured, while the larger vessels surrounding the tubes resist the pressure. Later, these allow the transudation of fibrine, and finally may themselves give way in totality, causing hæmorrhage much more abundant than in the first case.

But Cornil and Rindsfleisch declare that these casts do not present the reactions of fibrine, and are formed by a colloid secretion from the epithelial cells, or from the débris of the cells themselves, agglutinated together. In this view they are analogous, not to the exudation of parenchymatous inflammation, but to the mucous secretion which accompanies the epithelial proliferation on inflammed mucous membranes. Hence Cornil resumes this exudative affection, with that characterized by the desquamation of epithelium, under the common title, Catarrhal Nephritis; and Dickinson, in the same way, classes them together as the several modes of Tubular Nephritis.

No special justification is required for the term inflammation as applied to these lesions. The entire range of pathology may always be ransacked at any moment that it becomes necessary to define a lesion as inflammatory. To avoid this excursion, it may here suffice to adduce the pain, fever, exudation, and prolifertion of tissue (epithelial cells), as the assemblage of characters whose ensemble is usually defined as inflammation. The terms Parenchymatous Nephritis, Nephritic Albuminuria, are applied by the best modern writers in virtue of this well-founded analogy.

By extension, however, the same term is retained, when the

[1] The term croupal is used by German pathologists to characterize inflammation attended by fibrinous exudations.

only character of inflammation present is the proliferation of tissue. And this frequently occurs.

For although epithelial casts are constantly present in acute febrile albuminous dropsy, the converse is by no means true. The disease, although clearly traceable to exposure to cold, may creep upon the patient silently and insidiously, manifesting itself by slight œdema about the ankles and puffiness of the face, without any fever, lumbar pain, difficulty of micturition, or unusual appearance of the urine calculated to fix the patient's suspicion upon the kidneys as the seat of disorder. Chemical and microscopical examination of the urine, however, discovers it to be decidedly albuminous, and containing blood-corpuscles, free epithelial cells, hyaline casts, and epithelial cylinders. Or these latter may succeed to simple albuminuria, without any exacerbation of the general symptoms. In this case it is probable that the desquamative and exudative lesions are confined to a comparatively small number of tubes, and are insufficient to excite the nervous disturbance upon which pain and fever depend.

In other cases, these seem to be escaped in virtue of the unusual extent of the tubular obstruction, which, by interfering with the elimination of urine and urea, determines a drowsy, apathetic condition, in which the sensibility and capacity of reaction are equally diminished.

The frequent succession of the foregoing alterations in the composition of the urine indicates the frequency with which one kind of lesion of the vascular element of the kidney may be followed by another kind of lesion of the glandular. The possibility of the sequence is fully established, and our second question answered before our first. To establish the necessity of this sequence is not so easy, because so many cases of albuminuria, in which the urine is loaded with epithelial cells and casts, do not come under observation until long after the *debut*, and have been ushered in by no acute febrile attack, or even congestion sufficiently violent to notably affect the color of the urine. All analogy shows, however, that congestion sufficient to greatly modify the nutrition of a part, and determine the proliferation of its epithelium, may be insufficient to cause the rupture of the blood-vessels. The supposition, so widely admitted, that the hyaline cylinders are formed of coagulated fibrine, exuded from the blood-vessels under increased pressure, necessarily implies

congestion as the mechanical condition of their formation. The theory of cell-secretion implies it by analogy, as the vital condition of increased activity. Finally, although the delicate Malpighian plexus probably ruptures under this amount of pressure, resisted by the tubular vessels, the exudation in the tubes may be sufficient to prevent the escape of so minute a quantity of blood, or only permit the gradual filtration of the blood-corpuscles. Dickinson relates a remarkable case of acute desquamative nephritis, supervening after scarlet fever, in which the tubes were so completely blocked up as to cause the nearly total suppression of urine, and entirely prevent the passage of blood, although, after death, the kidneys were found in a state of intense congestion. The writer justly observes, that in severe cases of this form of disease the absence of hæmaturia is a much more dangerous sign than its presence.

From these considerations, therefore, and in the absence of any direct proof to the contrary, we may presume that the sequence between vascular congestion and catarrhal nephritis, or rather the dependence of the latter upon the former, is not only possible but necessary. Congestion may stop short of desquamative nephritis; but this always originates in some degree of congestion—a fact that confirms the inflammatory nature of the desquamation.

If the symptoms progress in severity, or simply present induration, the morphological analysis of the urine is nearly always complicated by the appearance in it of fat globules. Their further description must therefore be confounded with those characteristics of the

3d Class—Fatty Degeneration, etc. The pathognomonic sign of fat deposit in the uriniferous tubes, is the presence in the urine of fat globules contained in epithelial cells or studding granular cylinders. We have already alluded to the circumstances which must be taken into account in a diagnosis based upon fatty urine. It is only when the epithelium containing the fat is shrivelled and granular, when the cylinders are not too black, and the free oil globules not too abundant, that the fat can be referred to that condition of the kidney known as fatty enlargement. The possibility of sequence between congestion, catarrhal nephritis, and fatty degeneration, as manifested by the successive appearance in the urine of blood, epithelial cells and casts, and fat

globules, is as well established as that between congestion and desquamation. All authors relate cases in which, after an acute attack of albuminuria and dropsy, ushered in as described above, the epithelial casts are replaced by hyaline tubes studded with fat, the blood disappears from the urine at the same time the febrile symptoms subside, the albuminuria and dropsy may diminish, the patient considers himself well. After an interval of some months, during which he complains of nothing but lassitude, and perhaps frequent vomiting, the patient's illusions are destroyed by a renewal of the dropsy, suddenly, after fresh exposure to cold—or establishing itself gradually and insidiously. The characteristics of the urine in these cases are the same as in the catarrh, with the addition of fat. It is diminished notably in quantity and in specific gravity—the latter corresponding to the diminution of urea. The dropsy is extensive from the beginning of the disease or of the renewed attack and death most frequently results from effusion, in order of frequence, into the cellular tissue, the peritoneum, pleura, or pericardium. These symptoms result, as in the case of simple desquamative catarrh, from the blocking up of the uriniferous tubes, in this case by an agglomeration of granular and fatty granular masses, crumbling cells, and cylinders of all varieties. Whenever—by means of powerful diuretics—a stream of urine is sent through the embarrassed tubes, the dropsy diminishes.

The same series of chronic symptoms, as we have remarked in the case of catarrhal nephritis, may develop themselves insidiously from the beginning; after months of ill health vaguely defined, or in the course of some chronic disease, œdema of the ankles may appear, and examination of the urine detect albumen, epithelium, and fat. But fat is never discovered during the first days of albuminous dropsy occurring after exposure to cold, etc.

On the other hand, we have seen that all cases of desquamative nephritis present an admixture of fat globules in the urine after a period of varying duration, and the general symptoms characteristic of this lesion cannot, after a certain prolongation, be distinguished from those of fatty enlargement. It is certain, therefore, that fatty degeneration may result from a destructive process consequent upon desquamative nephritis.

According to a theory once professed by Johnson as exclusive, and still retained by this distinguished writer as applicable to at

least a certain number of cases, the fatty degeneration of the epithelium is the initial lesion of a peculiar affection, distinct from inflammation, and originating in "an unsuccessful effort of the kidney to eliminate a superfluity of fat from the system, and the consequent fatty *infiltration* of the glandular cells." This infiltration—occurring as we have seen in the course of chronic diseases, when the fat left unappropriated by the slackened nutritive processes has a tendency to deposit itself temporarily in the epithelium all over the body—should explain the frequency of Bright's disease as a secondary affection.

But, on the one hand, this fatty infiltration differs distinctly from the degeneration, both in its anatomical characteristics and the symptoms to which it may give rise. The infiltrated epithelium is normal and perfect in structure, the oil globules are large, abundant, and distinct,—the outline of the cell firmly defined, the nucleus generally visible,—and there is no sign of the granular tumefaction—the "cloudy swelling," characteristic of cellular disease. The same peculiarities may be observed in the fatty epithelium that finds its way into the urine, where it is only present in small quantity, entirely disproportioned to the amount of fat deposited in the uriniferous tubes and unaccompanied by fatty granular casts. The amount of desquamation that takes place under the influence of the fatty infiltration only slightly exceeds that of health, and the vitality of the epithelial cells is not seriously injured by the deposit. This may also be completely absorbed, as from other epithelium, and Beer has observed the renal stroma, especially the angular spaces left between the conjunctive cells, entirely filled with fat globules, presenting appearances similar to those of the fat globules in the intestinal villosities during digestion.

"Within certain limits," observes Dickinson, "fat appears to be a temporary inmate of the epithelial cells."

On the other hand, the disease most influential in determining fatty infiltration—tuberculosis— has no marked connection with Bright's disease; indeed, Dickinson, after numerous autopsies, declares to have observed even a sort of antagonism between them. Conversely, the affections unanimously recognized as causal conditions of the morbus Brightii—valvular disease of the heart, gout, alcoholism, pregnancy, or syphilis, scrofula, suppurating osteitis—tend to produce these amyloid degeneration,

those interstitial nephritis, and have no influence in the formation of the large, smooth mottled kidney of fatty degeneration. It is probable, therefore, that Johnson's theory reposes on an inaccurate connection between facts, each in itself accurately observed,—fatty infiltration of the kidney, passage of a certain amount of infiltrated epithelium into the urine,—development of Bright's disease in the course of chronic extra-renal affections

P. C. M.

SOME DETAILS IN THE PATHOGENY OF PYÆMIA AND SEPTICÆMIA[1]

REMARKS BEFORE THE MEDICAL LIBRARY AND JOURNAL ASSOCIATION OF NEW YORK.

As I understand the object of this Association, it permits its younger members to submit the questions that may have especially interested them, to the judgment of others older and more experienced than themselves. This is why I venture to make some remarks on a subject, which a rather prolonged sojourn in the hospitals of a great metropolis has forced most prominently upon my own attention.

The study of putrid and purulent infection, though it orginate in the province of the operative surgeon, immediately rises into the sphere of general pathology, and touches upon problems of the keenest interest to the physician. Moreover, by a singular fatality, the methods adopted to remedy the effects of accidents, expose to many of the same dangers as the accidents themselves; and operations for many diseases of internal origin accumulate upon the unfortunate patient the additional perils of an external traumatism. Finally, while men are more especially exposed to the superfluous traumatisms of war, women are compelled to incur the inevitable traumatism of childbirth; and the ultimate danger is in many cases the same.

To judge by certain assertions, one might suppose that this danger had been so much diminished by appropriate treatment, that it now presented no more practical interest than that of the plague. The admirers of Professor Lister have far surpassed his own confidence in the virtues of carbolic acid. In the *Dublin Quarterly* for 1869, Mr. McDonnell affirms that treatment by

[1] Reprinted from *The Medical Record*, 1872. VII, p. 73.

Lister's method has attained *perfection;* and that when a wound has been dressed with carbolic acid, and its arteries secured by torsion, it is almost certain to heal easily, while the temperature of the body does not rise above 100°. Lister himself, in his famous paper published in the *British Medical Journal* for 1867, is not so sanguine. He gives no statistics, but observes that since the employment of carbolic acid for the dressing of wounds, his wards, formerly the most unhealthy in the Glasgow infirmary, have become comparatively healthy, and that he no longer dreads as before, the advent of compound fractures. In *St. George's Hospital Reports* for 1868, Mr. Holmes gives the result of experiments made with carbolic acid in forty cases. Lacerated wounds and abscesses did remarkably well under this treatment; all recovered. Of eight cases of compound fracture, four recovered and four died—two of pyæmia, one of tetanus, one, on fourth day, without metastatic abscesses, the case being complicated with renal disease. Mr. Holmes had twelve cases of incised wounds and operations, including two amputations of the breast, but only two involving section of the bones. One of these was a Chopart's amputation of the foot; patient recovered after an attack of erysipelas; the other, an amputation of a metacarpal bone, followed by death from pyæmia. In this case the patient had diseased kidneys.

In the last October number of the *Archives de Médecine*, Dr. Labbée reviews the English statistics, and points out that, even in Lister's wards, the results of carbolic acid treatment are much less brilliant than was at first supposed. Thus, previous to this treatment the deaths after amputations were 41 to 126, or 1 in 3; after its adoption, they were 30 in 80 cases, or 1 in 2.6. During the last six months Dr. Labbée had extensively employed carbolic acid, with the most scrupulous attention to the details insisted upon by Lister, and nevertheless nearly all his amputated patients had died. This lugubrious statement corresponds entirely with the facts that I have had an opportunity of observing closely. For at least three years the use of carbolic or phenic acid has been almost universal in the great surgical wards of the Paris hospitals; but the mortality has not been notably modified, and remains higher than that of London.

These remarks, with no pretension to statistical value, are merely intended to show that the antiseptic treatment is by no

means infallible. The phenic acid of the moderns is not a panacea, any more than the famous sage of the ancients. It follows that the doctrine upon which its employment is based, is not sufficiently comprehensive to include all the cases to which it is applied. This doctrine is sometimes announced in a proposition that rather unfairly combines the opinions of Roser on miasms, and of Pasteur on animal germs. It is sometimes said that these germs constitute hospital miasms, and act either directly, being themselves absorbed into the blood, or indirectly, after having determined the putrefaction of traumatic fluids. The absorption of fluids thus putrefied is sufficient to account for the most various surgical accidents—phlebitis, erysipelas, pyæmia, septicæmia, hospital gangrene. In virtue of the assumed unity of their ætiology and of their nature, these several affections are asserted to be equally amenable to a single mode of treatment. This consists in the destruction of animal germs, first in the atmosphere, by purification of the air; second, upon the wound itself, by dressings with carbolic acid.

But, in the first place, the animal germs to which Pasteur attributes the mechanism of putrefaction do not exist merely in impure air, but even in tolerably pure air, unless it be absolutely free from animal or vegetable dust—such air, indeed, as may be brought from the summit of Mt. Blanc. To prevent putrescible substances from putrefying, it has been shown necessary to seal them hermetically, so as absolutely to exclude air. Donné experimented on an egg, which he carefully enveloped in cotton wool, and then obliquely pierced by a knitting-needle, previously heated to destroy any germs that might be clinging to it. By means of this puncture air was admitted to the interior of the egg, but only after having been filtered by the passage through cotton. The egg putrefied, as it will not do if it be covered with an impermeable varnish. But Pasteur asserted that the precautions taken had not been sufficiently severe; that it was possible to admit some of these ubiquitous infusoria during the manipulation of the experiment. How much more, then, in any wound not treated by absolute occlusion!

In the second place, the researches of Béchamp and Estor, communicated to the Académie des Sciences in 1868, indicate that, though the access of air be essential to putrefaction, yet the presence of bacteria in animal putrefying substances does not

depend on their introduction from without, but on the development of germs already contained in the elements of the tissues themselves. Especially in the cells of the liver, both in man and in other animals, these observers have discovered certain spherical granules which they call microzymes. These remain spherical as long as the organ is in health, and constitute a normal necessary part of its elements. But when the tissues are separated from their vital connections and exposed to the air, these granules at first arrange themselves in strings, and finally assume the shape of moving or motionless rods, presenting all the appearance of the vibriones known as bacteria and bacteridia, and described by Davaine.[1] Fragments of liver placed in water containing either sugar or starch, showed a development of bacteria in twenty-four hours. But if immersed in ordinary water, the development was delayed five to thirteen days. Further, what is extremely important to notice, the addition of creasote or of phenic acid in no wise affected this proliferation of animal germs, unless it were made in a quantity sufficient to coagulate animal tissues. That these bacteria came from the microzymes into which they were seen to grade, and not from the surrounding air, seems proved by the fact that they were always discovered in the fragments of tissue before they appeared in the water in which these were immersed.

In the *Quarterly Journal of Microscopical Science* for last October, Burdon-Sanderson has shown, by most careful experiments, that the microzymes never come from the air. According to this writer, they frequently abound in water, so that a drop or two of ordinary spring water added to a test solution is sufficient to determine in it the development of microzymes and of bacteria. But if the water so added be boiled and the whole placed in a glass that has been superheated, no microzymes will appear, even though the liquid be left exposed to the air. In the latter case, however, torula-cells appear in as great abundance as if the liquid had not been boiled. It is thus shown that no connection exists between the microzymes destined to develop into bacteria and the torula-cells that multiply into fungi. The air is charged with these latter, while the former abound in animal and vegetable solids and fluids.

To test the influence of bacteria, apart from that of the morbid

[1] *Dictionnaire des Science Médicales*, Art. "Bacteries."

fluids with which they were associated in Davaine's experiments, Leplat and Jaillard[1] injected the veins of animals with fluids containing bacteria obtained from decomposing vegetable infusions. Such injections were productive of no inconvenience whatever. Analogous experiments were made by J. G. Richardson, as related in the *American Journal of Medical Sciences* for July, 1868, p. 291. He swallowed from one to four fluid ounces of water, rendered putrid by two or three days' contact with meat, and swarming with vibriones. A drop of blood drawn half an hour after the ingestion of one fluid ounce, presented only a single vibrio. But with a larger dose and an hour's interval the number greatly increased. Twelve were seen in as many minutes, and at one time there were three in one field. In two hours, however, these had entirely disappeared, their presence in the blood having occasioned no other inconvenience than slight headache, furred tongue, and some diarrhœa.

Thus, left to themselves in contact with healthy living tissues, vibriones are rapidly eliminated without causing any damage. Moreover, as Davaine observes, in a medium composed of substances in full putrefaction, these animalculæ are also unable to sustain life. They live upon organic matter that is just beginning to decompose; they cannot determine the decomposition of living tissues; their germs cannot develop to the potency of bacteria unless they have free access to oxygen; finally, when the organic matter in which they are imbedded is dead, and resolved into inorganic elements, the vibriones die too. Hence, though Feltz and Cohn have discovered them in the fluids of putrefying wounds; though Ranvier has found them *near* the seat of a fractured bone affected with osteo-myelitis; though he has further found germ-granules in metastatic abscesses,—yet, these marvellous little organisms cannot be rendered directly chargeable for all the accidents of putrid and purulent infection; nor their destruction be considered an assurance of security against these formidable complications of wounds. For, 1st, in air that has been sufficiently purified to avert certain forms of disease, as hospital gangrene and the more malignant kinds of septicæmia, the germinal matter of vibriones is still found in considerable abundance, so that animal fluids or tissues exposed to the air necessarily decompose.

[1] *Comptes Rendus de l'Académie des Sciences*, 1867.

2d. Positive experiments have shown that bacteria by themselves, though introduced into the blood, are not injurious. Further: Bergmann has shown that the dried residue of pus retains its toxic properties, though it have been heated to 212°, or treated by alcohol of 96 per cent.; and either procedure is accounted sufficient to destroy animal germs.[1]

The above-quoted experiments of Burdon-Sanderson tend to show that the vibriones of the air, which are a cause of putrefaction, differ essentially from the microzymes of animal fluids, whose development may be only an effect. But the development of these latter to bacteria may act like that of all other vibriones, in favoring putrefaction. Hence the accession of air to a wound would work in two ways: it would admit atmospheric germs, demonstrated agents of putrefaction; and it would furnish the oxygen requisite for the development of microzymes, probable agents of putrefaction. The one and the other class of vibriones —innocuous if themselves absorbed—only act by determining the alteration of traumatic fluids; and these once altered become toxic, though the causal germs be excluded or destroyed.

That the accidents resulting from wounds depend upon the introduction into the economy of substances formed upon them is shown: (*a*) because the artificial introduction of these same substances, by injection into the veins, is followed by the same symptoms as occur spontaneously when they are left long in contact with living membranes; (*b*) because direct experiment proves that the capacity of absorption from the surface of wounds is very great.

Two great classes of infection may be formed: 1st. That which depends upon primary absorption from the wound previous to the growth of granulations or the formation of pus; 2d. That which supervenes after suppuration, and the complete disappearance of traumatic fever. The first class constitutes septicæmia: the second, pyæmia, or purulent infection.

It is noteworthy that septicæmia is developed at a time when absorption from the wound is slowest; pyæmia, when it has begun to be most active. Demarquay has shown that an iodic solution placed on a recent wound will be absorbed in from fifteen

[1] On this last point, however, there is at least room for doubt, since Wyman's experiments (*Am. Jour. Sci.*, 1867) have shown that certain infusorial germs will retain their vitality even after four hours' boiling.

minutes to an hour. But after application to a granulating wound, iodine may be detected in the urine and saliva in ten, eight, six, or even four, minutes. It would seem, therefore, that the accidents of septicæmia depend on the absorption of some substance more diffusible than that which is the origin of pyæmia, since it acts more quickly, though at a time when absorption is less active.

The wounds in which pure septicæmia originates may affect exclusively the soft tissues. Thus, in wards where pyæmia was the most frequent disease, I have seen septicæmia develop as a consequence of an operation for a double prolapsus of the uterus and rectum, where an attempt was made to extend the perinæum backwards by a suture that should include a part of the enormously distended anal sphincter.

Septicæmia is frequent after operations for strangulated hernia or the extirpation of tumors, and often complicates the peritonitis occasioned by ovariotomy. But in wards where pyæmia and septicæmia were both endemic, I have been struck with the exemption of patients who had suffered amputation of the mammary gland. On the other hand, the extirpation of a fibro plastic tumor from the deltoid muscle was followed by a typical development of septicæmia. The traumatic fever set in within twenty-four hours after the operation, and, instead of abating, persisted, and gradually rose into all the violence of the septicæmic fever.

This form of surgical fever is frequent as a consequence of diffused acute phlegmon of cellular tissue, even when this is unaccompanied by osteo-myelitis. It constitutes those peculiarly malignant forms of puerperal fever where death supervenes with great rapidity, and where, after death, the uterus presents no trace of phlebitis or of lymphangitis. These cases are exceptional.

It is a very remarkable circumstance that gunshot wounds, so excessively dangerous when they affect the bones, are so frequently innocuous when confined to the soft tissues. I had abundant opportunity for observing this fact, in the case of numerous shell wounds that came under my observation during the siege of Paris. In wards where acute diffused phlegmons frequently proved fatal by generating septicæmia, flesh-wounds caused by the explosion of shells healed readily, even though,

as in one case, the victim was a woman six months pregnant, and miscarried after a triple injury in face, thigh, and leg.

A curious case of impunity, even though the bone was involved, was that of another woman who had been for four years an inmate of the hospital on account of chronic rheumatism in shoulder, wrist, and knee-joints, all of which were more or less completely ankylosed. During the bombardment, a shell exploded in the hospital ward, and carried off this patient's right arm about three inches below the shoulder-joint. It was a very clean amputation, with very slight hæmorrhage; and but little trimming of the wound was needed to make a neat stump, which was speedily covered by fleshy granulations. Not merely did the patient escape without any signs of septicæmia or purulent infection; she even had no traumatic fever. I attributed this remarkable exemption to the pre-existence of chronic adhesive inflammation, which had rendered the tissues—lymphatic, cellular, osseous, and to a certain extent even the veins—impermeable to the septic material arising from the traumatism.

The behavior of gunshot flesh wounds resembles that of those made with caustics as compared with those made by the bistoury. It is not my province to insist upon the practical advantages of the method so eulogized by Maisonneuve, but their bearing upon the theory of septic and purulent infection is of importance. The facts tend to show that tissues killed outright, by chloride of zinc or the actual cautery, present far less chances of infection than those that die slowly and, during a long period, offer to absorption the successive products of their decomposition. They appear, moreover, to block up the roads of absorption, and not to afford the media for diffusion constituted by loose diffluent tissues.

Septic absorption occurs to a greater or less extent whenever decomposing animal matter is brought in contact with living membranes, and seems to be independent of the state of the veins.

The condition of the lymphatics is of much more importance. For at the time that septicæmia develops, the lymphatics, torn by the traumatism, gape open into the wound, while later they are closed by fleshy granulations. Septicæmia, which precedes the formation of these granulations, likewise, in typical cases, precedes the formation of pus. This does not normally occur

before the third day; and if symptoms of septic poisoning have appeared previous to its establishment, suppuration may be indefinitely delayed, or the pus be replaced by a thin sanious liquid, in which pus-corpuscles are rare. According to Robin, the decomposition of pus is always a consequence of the generalized infection, and not its cause.

As might be inferred from this apparent connection with the lymphatic system, the full development of septicæmia is often preceded by a lymphangitis. But it is extremely curious to notice that, should this lymphangitis be immediately followed by tumefaction or phlegmon of the nearest lymphatic glands, or by an attack of erysipelas, the general infection seems to be averted. I have in my notes three cases of compound injuries of the fingers, followed by abscess in the axilla, which in one case had been preceded by a subacute phlegmon of the arm; in another, by the red streaks of superficial lymphangitis; in the third, by a probable affection of the deep lymphatics. All these cases resulted in recovery, in the same wards where patients affected with quite similar injuries were constantly succumbing with symptoms of septico-pyæmia. The occurrence of erysipelas seems also frequently to ward off the graver affection. In two cases of carbuncle treated by extirpation, the occurrence of a chill, in connection with a sudden drying and glazing of the wound, was regarded as an ominous forerunner of septic infection, until the advent of a local erysipelas unexpectedly changed the prognosis. In another case, an operation for strangulated hernia in a woman was followed by an attack of erysipelas on the face, from which the patient recovered. She subsequently succumbed to exhaustion, but never presented either symptoms or lesions of putrid or purulent infection. These facts, and certain statistics showing the epidemic alternation of erysipelas with graver surgical affections, bear testimony in favor of the theory that ascribes erysipelas to a diffused inflammation of the lymphatics of the skin, caused by the passage through them of irritating substances. It is upon this theory that Maisonneuve has based his treatment of erysipelas by application of a blister directly to the inflamed surface, for the purpose of drawing off septic material in a profuse discharge of serosity.

The succession of symptoms in septicæmia generally occurs as follows: The traumatic fever, instead of abating, persists, or

abates only imperfectly. The suppuration, that should have set in on the third day, is delayed; and sometimes the wound becomes glazed and dry, or points of gangrene appear in tissues that seemed at first sufficiently vitalized. Sometimes, as was the case with the patient submitted to a perinæorrhaphy, a fugitive local erysipelas appears, to disappear after a few hours. About the fourth day occurs a single chill, often quite violent, immediately followed by a notable rise of temperature. By the fifth day the sutures in the wound give away, and the tissues begin to melt down into an increasingly putrescent detritus. At the same time the pain of the wound is diminished, and this local blunting of the sensibility rapidly extends to the entire nervous system. The patient becomes absorbed, indifferent, finally agitated and delirious. The delirium is sometimes muttering, often violent. The fever is remarkable for its continuance; morning remissions are slight. Under this continued fever the body emaciates; the cheeks become excavated, and covered with a dry parched flush; the eyes are injected; the tongue and lips retracted and blackened with fuliginosities; the whole aspect of the patient recalls that of typhoid fever, and the occurrence of diarrhœa completes the resemblance.

The peculiar circumstance about the pathological anatomy of pure septicæmia is its negative character. There is no trace of phlebitis, thrombi, or metastatic abscesses. The veins are all permeable, but filled with diffluent black blood like molasses. The viscera are nearly all softened and congested, as in typhus.

In artificial septicæmia, induced by the injection of putrid matters into the veins, there is often diffused pneumonia, or there may be patches of gangrene in the lungs. The most notable lesion, however, exists in the intestinal mucous membrane, which is tumefied, hyperæmic, and softened. This lesion corresponds to the sanguinolent diarrhœa, which is an invariable symptom during life, and both lesion and symptom point to an effort at elimination of the poison by the intestinal tube. That the lesion is so extremely marked in animals, while it is slight or wanting in human victims, indicates that the effort at elimination is greater in the former case than the latter, and helps to explain, therefore, the greater resistance of dogs to the disease.

Finally, a very notable peculiarity of septicæmia is its variable degree of intensity—variable as the conditions which may give

rise to it. There is the terrible septicæmia of malignant forms of puerperal fever, that destroys life in a few days; there are the much milder forms, that almost invariably occur when decomposing animal fluids are brought in contact with any surface of the body in such a way that any part of them may be absorbed. The retention of a piece of the placenta in the uterus will give rise to all the symptoms of an incipient septic fever, as I had an opportunity to observe in a case the other day. Nay, even in perfectly normal conditions, recent accurate observations have noted a rise of temperature as a general occurrence about twelve hours after parturition. This phenomenon is precisely analogous to the well-known traumatic fever, and the elementary conditions are the same, namely, the contact of decomposing non-purulent fluids with living membranes capable of absorption.

In these cases, the general disease seems to be directly connected with the wounds, and this, not in virtue of the nervous shock they have inflicted, but of the decomposing liquids, or of some element in them, that they place in contact with live animal membranes. The essence of the general disease lies in the fever, or rise of temperature; and of all the causes that have been invoked to explain the rise of temperature in such cases, that of an acceleration of the molecular metamorphoses of the blood and tissues is infinitely the most probable. For the fever may be determined, either, as I have said, by contact with animal membranes of substances themselves undergoing rapid chemical metamorphoses, or by an injection of these same substances into the blood, as in Billroth's experiments.[1] In these experiments there was no chill, but the temperature rose immediately as high as 40.5°C., and other symptoms of septicæmia followed, as diarrhœa and great prostration. Recovery frequently took place. In some cases, where the amount of injected material had been very large, death occurred, and then the only visceral lesions discoverable were diffused congestions, especially of the intestinal mucous membrane. From these experiments the gravity of septicæmia is shown to be in direct proportion to the amount of putrid matters thrown into the blood, although in every case they determined a rise of temperature. Below a certain limit of quantity, they could be supported; but above that limit, they occasioned more violent symptoms, which finally proved fatal. This grad-

[1] *Archiv für Klinische Chirurgie*, 1862.

ation corresponds to that furnished by clinical experience—to the immense variety in the severity of septicæmic symptoms, which are least of all after normal parturition; greater after wounds, and in proportion to their extent; greatest of all when, long after the original shock, new tissues have sloughed by the invasion of hospital gangrene. There are, therefore, the strongest reasons for accepting the recent doctrine of Billroth, which interprets traumatic fever as a form of septicæmia, and septicæmia as a simple extension or aggravation of traumatic fever.

This analogy suggests that of numerous other affections in which septicæmia plays a prominent part, as hospital gangrene, carbuncle, malignant pustule, typhoid fever, variola, ulcerative endocarditis, even erysipelas when the effort at cutaneous elimination has proved unsuccessful. In all these cases the evolution of the affection seems to be connected with the presence in the blood of rapidly decomposing substances, whose metamorphoses accelerate those of the animal tissues, including the blood, and thus raise the temperature of the body.

It is in virtue of this rise of temperature that septicæmia is allied to pyæmia; for the injection of pus into the veins, even when it produces no other effect, generates fever as intense as that produced by the injection of putrid non-purulent fluids—fever which may end in death. In other respects the symptoms and march of pyæmia are quite different from those of septicæmia.

As every one knows, pyæmia originates most frequently in some lesion of bones, which places the wounded osseous tissue in immediate communication with the air. The larger the bone, the greater the danger, which is most to be dreaded after injuries of the femur. Who is not familiar with the train of events that, in such a fatally large number of cases, follows upon an amputation of the thigh? The patient may have recovered from the initial traumatic fever, and on the fourth or fifth day be apparently in very good condition. Suppuration, which set in on the third day, becomes profuse, but the pus remains thick, yellow, laudable, or only slightly offensive in smell. Thus, suppuration precedes the invasion of pyæmia, while the symptoms of septicæmia generally begin before the establishment of suppuration, or arrest it if pus be already formed.

The wound is extremely painful, and its sensitiveness seems to increase instead of diminishing. The process of dressing the

wound throws the patient into an agony; but when this is over, he is comparatively comfortable, often extremely hopeful and sanguine. One day, generally between the fifth and tenth, the nurse observes that the patient has eaten less than usual. In reply to inquiries, however, he asserts, sometimes quite vehemently, that he is perfectly well. A day or two later he acknowledges having had a slight chill or fever, that he refers to only on account of the persistent questionings of the surgeon. Sometimes several days, even a week, will elapse before the chills are repeated; sometimes they follow in rapid succession, coming every day, or even twice and thrice a day. It is extremely rare that only one chill precedes the invasion of fever; and even then, this invasion only occurs after a certain interval. The chills are very apt to return after the establishment of fever, and checker its course in a way never seen in pure septicæmic infection.

The rise of temperature occurs sometimes after the complete subsidence of the traumatic fever, in notable contrast with that of typical cases of septicæmia. It is rarely as rapid, as intense, or as continued as in the latter case; for some time it presents very marked morning remission, and only gradually rises into continuity.

The appetite and strength fail, and the patient is conscious of his increasing weakness, and complains of it, as is never the case in septicæmia. Hence it seems much more notable. The skin and sclerotics assume the characteristic yellow hue, extremely unlike the red parched flush of septicæmia, and analogous to that of cancerous infection, with which Billroth compares it. It is one of the forms of jaundice described by Gubler, dependent on a destruction of red corpuscles in the blood. The tongue of the patient becomes dry, but never blackened by fuliginosities as in septicæmia. The intelligence remains clear, but the mind begins to be darkened by gloomy forebodings, by a semi-consciousness of the dissolution of vital forces already begun.

During this time the aspect of the wound is little changed, the suppuration is abundant, according to Billroth is increased, and my own observations accord with this statement. An extension of suppuration takes place, as the older writers would say, and is manifested in one or more of three different ways.

In the first place, cold abscesses may form in different parts of the cellular tissue, generally of the extremities, sometimes in the

more decumbent portions of the trunk. In the second place, purulent effusions may take place into the articulations, or serous cavities, and that with extraordinary rapidity. This purulent arthritis is a very common manifestation of infection during the puerperal state. I remember one case especially, which I had an opportunity of observing at the clinique, that might be considered as a type of this class. The patient had had an attack of subacute metritis, which was subsiding, when she complained of pain in her left elbow-joint; and in the course of twenty-four hours the articulation became evidently filled with liquid, swollen, extremely tender, but without any inflammatory redness. The only other symptoms presented by the patient were diarrhœa, and a certain dulling of the intelligence. M. Depaul immediately pronounced a fatal prognosis, which was justified four or five days later by the death of the patient with all the symptoms of purulent infection.

These external suppurations, however, indicate a comparatively curable form of the disease, and, when manifestations of the infection are confined to the cellular tissue or the articulations, the patient may recover. I have seen three curious cases of this category. After a compound fracture of the tibia, a patient was affected with cold cellular abscesses in various parts of the body, and with purulent effusion into one of the knee-joints; yet he ultimately recovered.

In another case, also of compound fracture of the tibia, the patient had had three chills, and an abscess had developed in the thigh. After the administration of quinine, the course of the infection seemed to be arrested, and the patient recovered.

In a third case, the purulent infection had originated in an anthrax, which had been followed by abscesses in the breast, glutæal region, and leg, and by an effusion probably purulent, in the knee-joint. Notwithstanding this multiple suppuration, the patient recovered, and the articular effusion was reabsorbed with the rapidity so remarkable in such cases.

These cases recall the experiments of Sédillot upon dogs, where injection of pus into the veins was followed by the development of external abscesses, but finally by recovery.

External suppuration or suppurative inflammation of serous cavities may, however, of itself prove fatal, as in a case that I saw

at the Children's Hospital, where purulent infection is comparatively rare. A child four years old had been submitted to Chopart's amputation of the foot, on account of a fungous arthritis, accompanied by necrosis of some of the medio-tarsal bones. The first few days after the operation passed very well; then secondary fever set in, and was followed by symptoms of arthritis in both elbow-joints, and of double pleural effusion. Death occurred about a fortnight after the operation, and at the autopsy were found abundant purulent effusions in the articulations, in both pleural cavities and in the peritoneum. This purulent peritonitis had been latent and quite painless, and had probably taken place during the last days of existence, when sensibility was blunted. There were no traces of metastatic abscesses in lungs, liver, or spleen.

These metastatic visceral abscesses constitute the third form of generalized suppuration, and the lesion most characteristic of pyæmia. The invasion and march of the disease seems to coincide exactly with their development and evolution. To them is due the dyspnœa that occurs early in the disease, while that of septicæmia, dependent on the poisoning of the mass of the blood, does not supervene till later. The respirations are rarely below forty, sometimes as high as fifty or sixty. With the progress of one or the other of these suppurations, the prostration of strength increases. All the powers of life seem to be gradually dissolved apart from one another, and drift away separately before sinking down into the sea of nothingness that is rising to engulf them. Hence towards the close, a peculiar incoherence of the mental faculties and of speech, that is quite distinct from the delirium of septicæmia. The patient will interpolate absurdities in the midst of a conversation whose general tenor is reasonable; he has temporary hallucinations of vision; he loses all capacity for comparison, and consequently for astonishment; his mind resembles that of a person in the incipient stages of dementia.

This ataxia of the intelligence finally extends to the spinal and peripheric nervous system; there is carphologia, the wandering involuntary movements by which the patient seems vainly endeavoring to clutch at the life that is slipping away from him. Finally, consciousness is entirely gone—the patient lies on his back with his eyes closed, already half a corpse. I have seen one case where this condition was prolonged a month, and Mr. Paget

relates similar cases of chronic pyæmia, where, as in Edgar Poe's story, the patient seems to have been magnetized when at the point of death, and his dissolution arrested, but left constantly imminent. It is during this last period that the suppuration on the wound diminishes.

The autopsy of patients who have succumbed to pyæmia reveals one or other of three characteristic lesions, and very frequently a combination of all of them: osteo-myelitis; venous thrombi, with or without phlebitis; visceral abscesses, or purulent effusions into the articulations or serous cavities.

The osteo-myelitis, whose presence might have been inferred from the constancy with which pyæmia is associated with lesions of the bones, is extremely frequent. M. Ranvier, in an article published in the *Lyons Médicale* for last May, observes that he found it in all the autopsies made at Val de Grâce during the siege. M. Gosselin, in his clinical lectures, always insisted upon this coincidence, and has referred to it again in the recent discussion at the French Academy. I have certainly had abundant opportunity to observe it myself.

This osteitis is always of the rarefying variety; the bony tissue is highly vascularized, and the surface of a section shows the trabeculæ to be red and softened, and the spaces they enclose enlarged, and filled with grumous sanguinolent matter. The medullary canal is filled with a vascularized pulpy mass, whence the fat has disappeared, a most noteworthy circumstance. A very characteristic detail is the projection of a portion of the mass from beyond the medullary canal.

Of less importance for the pathogeny of pyæmia is the thickening of the periosteum, and its frequent detachment from the bone by neoplastic material formed beneath it.

There are two noticeable facts about a bone in this condition: 1st. That all its cavities, both the medullary canal and the spaces between the trabeculæ, instead of being obstructed by solid clots, are filled with loose, pulpy, diffluent matter, exactly calculated to offer a medium of diffusion for liquids carrying solid particles in suspension. 2d. That the fatty matters of the medulla, so eminently adapted to rapid movements of chemical metamorphosis, are as ready for absorption as are the liquids on the surface of the original wound.

Not only is osteo-myelitis an almost invariable attendant on

fatal cases of compound fracture, but it may constitute the sole perceptible cause of pyæmia, as when the latter supervenes upon an acute necrosis (so called), or even, under certain circumstances, upon simple fracture. I have observed an example of pyæmia in an old man affected with simple fracture of the neck of the femur which had begun to consolidate when he succumbed to the affection, at that time endemic in the ward. Billroth quotes a similar case; and Prescott Hewett records a third in the *Lancet* for 1867.

In another instance that I have seen, the pyæmia supervened upon a chronic otitis, accompanied by caries of the petrous bone. In all these cases, metastatic abscesses were formed in the lungs.

The second lesion that may be found in pyæmic autopsies is phlebitis. Billroth observed it twenty-eight times in eighty-four cases. As is well known, it is to phlebitis that Berard ascribes all the phenomena of purulent infection. The apprehensions of danger from this cause have been much diminished since Virchow has shown that the coagulation of blood in the veins is not its consequence but its cause, and that irritation directly applied to empty veins inflames the external coat, but leaves the inner tunic untouched. The inflammation of the inner coat is always a consequence of the softening of the coagulum that has formed in the cavity.

This softening, or suppuration, as it has been called, takes place in two ways: 1st. By the molecular disintegration of the fibrin. 2d. By the penetration into its mass of pus cells, or white blood-corpuscles, that have wandered from blood-vessels or from purulent collections in the neighborhood of the thrombus. Hueter, in his chapter, in *Billroth and Pitha's Surgery*, does not hesitate to admit this penetration, and considers it proved: (*a.*) By Cohnheim's experiments on the capacity of white blood-corpuscles to traverse the walls of capillaries and blood-vessels. (*b.*) By those of Recklinghausen, published in the *Archiv* of Virchow, and which show the contractility of pus cells, and of cells of connective tissue. (*c.*) By the experiments of Bubnoff, recorded in the *Centralblatt*, of 1867, there has been directly observed the passage into the thrombus of pus cells that had previously taken up granules of cinnabar. In this connection we may inquire whether the presence of the granules did not communicate a force of impulsion to the cells that they otherwise

would not have possessed; and whether, therefore, pus cells that had become granular by commencing decomposition, would not be able to penetrate where others were shut out.

When, by the medium of a phlebitis, inflammation may be propagated from the wound to tissues of vital importance, the softening of the clot may prove immediately fatal. In the *Archives Générales* for 1871, Reverdin has pointed out that in anthrax of the face, inflammation of the facial veins may extend to the sinus of the dura mater, and excite a fatal meningitis.

Except in such special circumstances of contiguity, phlebitis remains a purely local affection, not only in such typical cases as that of inflamed varicose veins, but also in others where it seems to substitute itself for a general disease. Thus, during an epidemic of so-called puerperal fever, the patients affected with a well-defined metritis, or phlegmasia alba dolens, generally escape peritonitis or septic infection. Even the phlebitis that seems to have been generated by transport of pyrogenic material from a distant wound, may result in recovery, without signs of infection. I have seen one case where the extirpation, for cancer, of a mammary gland, was followed by phlebitis of the right leg—a real phlegmasia alba dolens, which retarded, but did not prevent recovery. So marked is the opposition between local adhesive phlebitis and pyæmia, that Sédillot treated incipient cases of the latter disease by cauterization over the veins leading from the wound, for the purpose of exciting inflammation and an effusion of plastic material that should erect a barrier against the absorption of putrid substances, or, as Sédillot maintained, of pus.

When the fibrinous coagulum remains hard, and obliterates the vein, no phlebitis ensues, as may be seen in ordinary varicose veins, or in the inopexia of cachectic diseases. The inflammation of the inner coat of the vein, as I have said, only occurs when the process of softening of the clot has extended the cavity first hollowed out in its centre, to the membrane limiting its periphery. It is infinitely probable that this process, which causes the phlebitis, is itself the cause of accidents of which the phlebitis is only an incident, because, on the one hand, these accidents coincide with the softening of thrombi, when little or no phlebitis exists; on the other hand, there are very rarely accidents with a less degree of softening, and a very intense degree of phlebitis.

The question of the influence of phlebitis in pyæmia resolves

itself, therefore, into two others: 1st. The formation of thrombi; 2d. Their disintegration.

Now it is extremely noticeable that many of the circumstances which favor the development of thrombi are precisely those which seem almost essential to the development of pyæmia. As Weber observes, since every traumatism involves a solution of continuity of veins, and since this is necessarily followed by a coagulation of blood in their interior, the formation of thrombi is a necessary consequence of every wound. But in superficial wounds of the soft tissues, the vessels are small, and easily obliterated at their extremities. When larger vessels are torn, one of two things happens: Either the vessel flattens together above the clot, obliterating its extremity, and then the thrombus never projects into a free cavity; or the clot is prolonged as far as the nearest collateral vessel, and its extremity floats free in the stream, and in a space larger than its own diameter. When this last occurs in arteries, as is normally the case after ligature, no harm results, because the stream of blood is not coming from the collateral, but passing down into it, and if any fragments are separated from the fibrinous clot they must be carried down to another point on the periphery of the vascular system. But in the case of a vein the conditions are reversed; the collateral current comes from the periphery, strikes the floating end of the coagulum, breaks off a fragment, and carries it towards the heart and lungs.

Of the conditions which favor the distintegration of the thrombus, the first, therefore, are those which expose its free end to be broken off mechanically. These conditions are two: length of the coagulum, and such a structure of the tissue surrounding the veins as is opposed to their collapse.

The length of the coagulum is increased: 1st. By whatever increases the amount of fibrin in the blood, or render its circulation sluggish, as abundant hæmorrhage, exhaustion from previous disease, privation, or old age. Weber attributes the rarity of true purulent infection among children to the activity of their circulation, which restricts the formation of thrombi. 2d. By the absence of valves in the veins. 3d. By the sudden suppression, through amputation, of an extensive vascular territory.

The two latter conditions are presented by the veins in the long bones, the last especially after amputation of the thigh,

where, as is known, the liability to pyæmia is at its maximum.

In osseous tissue, and especially in that of the long bones, is also found the second general condition favoring long coagula, namely, a structure that prevents the veins from collapsing. Lining the pores of the bones, they are necessarily maintained distended, and this circumstance, which has been wrongly supposed to imply such gaping open into the wound as should permit the direct entrance of solid particles, really favors the formation of lengthy coagula, with all their consequences. The same condition may be presented by the sinuses of the uterus when that organ fails to contract sufficiently after parturition. Here again are no "open mouths," but tortuous vascular canals, in which collateral currents meet and cross each other in every direction, filled with loose fibrinous clots that offer media for diffusion, and are liable to disintegration.

The circumstances that favor the disintegration of the clot throughout its mass, constitute the second class of conditions which render thrombi dangerous.

1st. First among these is the osteo-myelitis, which we have already noticed as so generally existing in fatal cases of pyæmia. The local activity of the circulation, uncompensated by sufficient force in the central part of the system, on the one hand determines serous effusions into the clots; on another, creates collateral currents ready to carry down stream the fragments resulting from the disintegration. This effect is added to that proper to the inflammation itself.

2d. The softening of the clot is favored by the penetration into its mass either of pus cells already effused elsewhere; or of white corpuscles directly passing from the neighboring blood-vessels; or, finally, of putrid liquids. We have noticed that the penetration of pus cells, which could not in any wise be considered a phenomenon of absorption, would be probably favored by their granular and angular condition; and as this is peculiar to decomposing pus, the influence of the latter may be in part explained. As to the white blood-corpuscles, with which we have seen that a certain proportion of pus cells may be identified, the most common condition of their transudation is their previous stasis in capillaries, which has long been known to be one of the initial phenomena of inflammation. The well-demonstrated influence

of intense local inflammation upon the development of pyæmic accidents, is thus in part accounted for by the penetration into the interior of thrombi of pus cells existing in the neighborhood. When such elements come from the tissues surrounding the thrombus, they penetrate it in virtue of the contractility demonstrated by Cohnheim and Recklinghausen. But, if they ever come from the surface of the wound—that is, penetrate from without inwards—they are diffused like other solid particles in the liquids which hold them in suspension.

Since, after the closure of the lymphatics, there are no open-mouthed vessels gaping into the wounds, and consequently the absorption of pus as such is impossible, this diffusion from the surface of the wound is regulated by the general laws of diffusion. Thus it will be greater when there is a stronger pressure without, such as may be exercised by tissues rendered tense by inflammation or by fibrous aponeuroses, and when this is combined with a diminution of the pressure within, as by a lowered tension in the blood-vessels. A liquid diffuses more easily when it is less dense, and especially when it contains less colloid or albuminous substances. Hence the thin sanious fluids first formed on wounds may be expected to diffuse more readily than thick pus, rich in albumen; and this corresponds to the fact already noticed, that septicæmia precedes the formation of pus, and seems due to toxic substances more diffusible. A granulated condition of the pus cells may favor their penetration. Finally, according to Sachs, the protoplasma of cells has a tendency to retain the solid particles of liquids that diffuse into them, so that these grow less and less dense as they traverse successive layers of cells. With each decrease of density, or diminution of solid particles, the diffusing power is heightened, and the fluids that have once traversed the outer barrier of tissues, pass with continually increasing rapidity towards the interior of central canals or vessels. The ultimate action, therefore. would almost necessarily be exercised by the fluids.

The fluid invariably present in cases of purulent infection, and added to the ordinary traumatic fluids generating septicæmia, is pus. From the numerous experiments that have been made by the direct injection of pus into the veins, certain facts may be considered as proved. 1st. A single injection of fresh pus causes a temporary rise of temperature, but no other inconvenience.

2d. This same rise of temperature may be induced by the separate injection of either pus globules or purulent serum. 3d. The injection of purulent serum holding inert solid powders in suspension, will determine the formation of pulmonary ecchymoses and infarcti if the serum be fresh; of pulmonary abscesses if it be beginning to decompose; of septicæmic symptoms and visceral gangrene if it be entirely putrid. 4th. The repeated injection of fresh pus at intervals of two or three hours will produce metastatic abscesses according to Sédillot; but according to Billroth, who repeated these experiments, only a rise of temperature sometimes, external suppurations, and occasionally pulmonary ecchymoses. 5th. Finally, the injection of putrid pus will determine a violent septicæmia, with patches of visceral gangrene, and, in a concentrated form, is one of the most violent poisons known. .012 of the dried residue is sufficient to kill a small dog.

The entrance into the economy of pus or of some of its elements, would seem, therefore, to act in one of three ways: 1st. As a medium for the diffusion of solid particles, which would block up the capillaries of the lungs, and thus form infarcti, according to the theory of embolism. These solid particles could not, however, be the pus globules themselves, because, in clinical conditions, these could never penetrate into the blood except in very small quantities, and a single injection of much larger quantities of fresh pus will not produce such effects. 2d. The pus may be supposed to act by a peculiar alteration of the blood, such as occurs by repeated injections at short intervals. 3d. Finally, it may act as a putrid fluid, but endowed with toxic properties of peculiar intensity.

The theory of embolism, invoked for the explanation of metastatic visceral abscesses, has, as every one knows, been successfully applied to the pathogeny of cerebral and pulmonary apoplexies; and there is no need in this place to enter upon its details. That the ecchymoses determined by the injections of fresh purulent serum and inert powders, represent the initial lesion of metastatic abscess, is rendered probable by the frequent clinical coexistence of the two, together with that of all the intermediate stages between them. Thus, in an interesting case of acute pyæmia, laid before the New York Pathological Society by Dr. Janeway, the lungs, liver, spleen, kidneys, and, extremely rare circumstance, even the heart, are said to be studded with

"minute white spots surrounded by a red areola," evidently metastatic abscesses. The first stage of such abscesses was represented in the lungs by numerous nodules of pulmonary apoplexy; the last, at the apex of the heart, by two small cavities entirely filled with pus.

But the progress of such infarcti towards suppuration, which is the characteristic fact of clinical pyæmia, and which is so difficult to reproduce in experiments, implies the addition of some other element to that of embolism; for ordinary infarcti do not suppurate, but undergo the fatty or cheesy degeneration. In pyæmia, therefore, either the embolus, or the liquids with which it is impregnated, or both, must be possessed of peculiar properties, and to them must be due the lobular inflammation of the lungs, and the suppuration in which it so rapidly terminates. This local inflammatory property may be considered identical with the general pyrogenic action that these same fluids exercise on the blood, and both depend on the accelerated metamorphosis of tissue and consequent generation of heat due to rapid chemical combinations.

Diffused suppurative inflammations are, therefore, unquestionably the proximate cause of visceral metastatic abscesses; and Ranvier, in his recent paper already alluded to, considers them sufficient explanation, without any necessity for the intervention of embolism. This distinguished microscopist declares that he never found the small vessels surrounding the abscesses to be obstructed; but on the contrary, during the ascending period of the abscess, they were always gorged with blood. These negative facts prove nothing, however, against the former existence of capillary embolics, which disappeared in the midst of the vascular afflux their mechanical and chemico-vital irritation had determined. At all events, these inflammations which M. Ranvier shows to be nodules of catarrhal pneumonia, with proliferation of epithelium, are very different from the diffused patches of congestion or gangrene determined by the injection of putrid matters, whether these be purulent or non-purulent. In the latter case the effect on the general mass of the blood overpowers the local effect on special groups of capillaries. The lesions, those of septicæmia, resemble those of typhus, while the catarrhal pneumonia of pyæmia resembles that induced by injection of solid powders into the bronchial tubes. The peculiar relations

of the capillary net-work of the lungs to the circulation, net-work which must necessarily be traversed by any solid particles circulating in the blood; the coincidence in pyæmia, of such conditions as render the circulation of such particles highly probable; the almost universal localization of pyæmic abscesses in the lungs where Billroth found them in seventy-nine out of eight-four cases; the coexistence of all grades of lesions intermediate, with ecchymoses and infarcti, and completely formed abscesses; all these facts indicate that the first-named mode of the action of pus—that where it acts as a medium for diffusing solid irritative particles—is one of the most characteristic, and that which chiefly distinguishes it from non-purulent septic fluids.

In the second place, the fact that repeated injections of pus will produce effects that cannot be determined by a single injection, imply that the pus may act by a previous special alteration of the blood again different from that of septicæmia. According to Sédillot, the effect of repeated injections of pus is to accumulate its corpuscles in the blood, and thus block up pulmonary capillaries by a peculiar kind of embolism. But this could not be true in clinical cases, for there the pus is not injected directly into the veins, but its elements, if they penetrate into the circulation at all, are diffused gradually in too small proportions to cause mechanical obstruction.[1]

Billroth admits a multiplication of the white blood-corpuscles, and an increased tendency on their part to accumulate in the capillaries of the lungs, whence the metastatic abscesses. He thus explains both the excess of white corpuscles noted by Sédillot, and also the peculiarity of the action of pus, as distinguished from that of septic non-purulent fluids.

Perhaps it is not too hypothetical to connect this excess of white corpuscles with the tendency to purulent effusions in the splanchnic serous cavities and in the articulations. These do not depend upon inflammation, for that seems rather to follow than precede them; and they may be reabsorbed with a rapidity unknown in ordinary purulent arthritis.

When the pus is putrid it acts like putrid fluids, and determines not pyæmia, but a septicæmia of peculiar malignancy.

[1] M. Demarquay has recently performed some experiments, considered to prove that such penetration *does* take place. See *Archives Genérales* for December.

The poison generated in pus, therefore, seems to be much stronger than that of other fluids.

Having passed in review the notable differences that exist between septicæmia and pyæmia in regard to their symptoms, the time and mode of their invasion, their anatomical lesions, and their reproduction by experiment, we are led to inquire whether these differences depend upon the operation of different poisons, or of the same poison operating in different conditions. The second doctrine is most emphatically affirmed by M. Verneuil in the recent discussion at the French Academy. He declares that pyæmia or purulent infection is to be regarded as an accidental complication of a general disease, septicæmia, which in a mild form, at least, exists necessarily in the case of every open wound. Septicæmia depends upon the absorption of sepsine, generated in the traumatic fluids; pyæmia occurs when this sepsine impregnates emboli that, carried to the lungs, form metastatic abscesses, of which each becomes a new focus of infection.

The term *sepsine*, invented by Bergmann and adopted by Verneuil, represents the unknown quantity existing in purulent or putrid liquids that gives them their peculiar pyrogenic properties. Great efforts have lately been made to isolate this hypothetical substance. Panum and Hemmer have shown that it exists partly in the serum of the pus, partly in the filtered globules. It is not volatile, and cannot be distilled from pus, but remains behind in the dry residue. An aqueous extract of this residue is toxic, and, moreover, will diffuse through animal membranes, and communicate its properties to pure water on the other side. This diffusibility is an eminent characteristic of the "sepsine," so called; and in virtue of it Bergmann claims to have isolated the toxic principle from many others with which it was associated, including inorganic matters, albuminous substances, and leucine.

Direct experiments with a number of substances, as various salts of ammonia, sulphide of carbon, solutions of leucine or tryosine, either produced no effect, or symptoms quite different from those of septic or purulent infection. With sulphide of ammonium alone, Weber, who employed much stronger doses than Billroth, obtained a notable rise of temperature and a septic inflammation of the intestinal mucous membrane.

Until the toxic principle in the two cases shall have been isolated its unity cannot be considered proved. In the mean time

the following facts speak in favor of the existence of two poisons analogous to each other, but not identical.

1st. That non-purulent liquids, holding solid particles in suspension, do not determine the same lesions as purulent serum that has first been filtered of its globules and then associated with inert powders.

2d. That the train of symptoms which occur after the establishment of suppuration, are not merely different in degree, but in kind from those which have preceded it.

3d. That pus exercises an action apart from that determined by its putridity.

4th. When pus is putrid it is a more violent poison than other putrefying traumatic fluids. Clinically the presence of putrefying pus is associated with the mixed disease, septico-pyæmia, the most fatal of all surgical affections.

5th. The conditions of the diffusion of pus have been shown to be different from those of primitive septicæmia.

Independently, however, of the probability of some special toxic agent in pus, which gives a peculiar character to pyæmia, the existence of metastatic abscesses introduces new complications by multiplying the foci of infection. From each abscess new pyrogenic material is continually being thrown into the blood, and when the abscess is situated in the lungs, the material that has been formed there is probably peculiar, on account of the peculiar exposure to air to which it has been submitted.

The dependence of the characteristic march of pyæmia upon metastatic abscesses, is shown by the insidious invasion of its symptoms at this moment of their formation, and by its gradual intermittent progress in proportion to their successive evolution. Hence the initial chill *after* the subsidence of traumatic fever; hence the increasing violence of the chills as the visceral suppurations become more numerous; hence the peculiar danger of pulmonary abscesses so much greater than that of even pyæmic abscesses formed in external cellular tissue.

On the other hand, it is unquestionable that the violence of pyæmia is not invariably in proportion to the number of pulmonary abscesses; and that in certain cases where the first symptoms have coincided with external suppurations, the abscesses found in the lungs after death are evidently of recent formation. I have seen several such cases, in which, even though

the final catastrophe be attributed to the pulmonary complication, yet it is unquestionable that pyæmia must have been prior to it. In these cases it is impossible to avoid belief in the direct action upon the blood of the elements of pus derived from the surface of the original wound.

The prophylaxis of septicæmia is more easily attained than that of pyæmia, because the conditions upon which septic poisoning depends are less peculiar than those of pyæmia. There is no special anatomical condition of the wound, such as exists when bony tissues are involved. The system is capable of tolerating the absorption of a certain amount of septic material, and when, from the extent of the traumatism, there is an excess of dead tissue, this may be removed by surgical interference. The great indications in the prophylaxis of septicæmia are 1st, to prevent the devitalization of new tissues; 2d, to prevent the exposure of tissues thus devitalized to the action of infusoria, or animal germs, the agents of putrefaction.

The first indication is pre-eminently fulfilled by purification of the air which the patient is compelled to breathe. This is effectual, not on account of any direct action upon the wound, but by maintaining the nutritive powers of the blood and its consequent action upon tissues whose vitality is threatened. So long as this vitality is maintained, the development of microzymes is to be little dreaded.

The second condition is only completely fulfilled when air is completely excluded from the wound, and with it, on the one hand atmospheric germs, on the other the oxygen necessary to the development of animal microzymes. Air is nearly excluded when a wound is enveloped in an atmosphere of carbolic acid, and to such exclusion must be largely attributed the favorable result of such treatment. Occlusion, either by this so-called "antiseptic" or by mechanical means, may prevent the decomposition of traumatic fluids; but when this has once set in, nothing will arrest it, not even the destruction of the germs which may have been its cause, as we have seen, these die of themselves in the putrefying fluids whose putrefaction they have determined. Hence a treatment directed to their destruction would be worse than superfluous, if it led to neglect of the great indication in the prophylaxis of this stage, removal of the traumatic fluids from beyond the reach of absorbents.

The greater facility with which this removal may be effected in the case of wounds of soft tissues explains their greater immunity from danger, and the far greater success of carbolic acid in their treatment. The septicæmia that may occur in the course of such wounds, depends upon the absorption of non-purulent decomposing fluids, and its intensity is in proportion to their mass. When the fluids already formed have been carefully washed away, the use of carbolic acid, that will not, by the ordinary methods, *prevent* decomposition, nevertheless restrain it, and hence reduce septicæmic accidents to a degree of intensity bearable by the economy. It is in these cases that the effect of good atmospheric hygiene is so apparent, by preventing the devitalization of new tissues. The most dangerous degrees of septicæmia may be averted, and its worst form, hospital gangrene, be entirely banished from hospital wards.

But pyæmia is connected with much more complicated conditions, many of which are entirely beyond the reach of carbolic acid. It depends on the presence of a fluid that, once secreted, decomposes with peculiar facility, seems to generate a poison of peculiar intensity, and expose the products of its decomposition to absorption at a moment that the power of absorption is at its maximum. It has been shown to act, not merely in virtue of such decomposition, like all putrid fluids, but by a special effect on the blood, and by a special connection with the thrombi blocking up the veins surrounding the wound. While the poison of septicæmia is principally absorbed by the lymphatics, that of pyæmia passes almost exclusively by the veins, either those in the bones, or those newly developed in the granulations of the wound.

This poison is therefore contained in inaccessible canals, and carbolic acid has as little effect upon it as it would upon an abscess in whose cavity it had been injected and shut up to mix with its contents. A comparatively minute proportion of purulent poison is capable of inflicting all the injury possible, so that great diminution of the mass of toxic material has very much less effect on the development of pyæmia than on that of septicæmia. The control must be complete, or it is useless, and it must be exercised not merely in the general hygienic conditions to which the patient is submitted, but still more, over the local conditions peculiar to the anatomical nature of his wounds.

From this predominance of local conditions, pyæmia is, as

might be expected, no exclusively hospital disease. Billroth asserts that it is as common in private practice as in hospitals, only when a death occurs that cannot be explained by hospital miasms, it is attributed to gastritis, or other accidental complication. Mr. Callender, in the fifth volume of St. Bartholomew's Hospital Reports, shows that although the mortality of city patients operated on in city hospitals was higher than that of the country, yet the mortality of country patients was the same, whether they were in large city hospitals, in small country hospitals, or even in average country practice.

It follows that absolute exclusion of air from the wound is much more important in the prophylaxis of pyæmia than of septicæmia. The partial occlusion effected by incrustation with carbolic acid, is often sufficient for wounds of soft tissues. But for wounds involving osseous tissues, the apparatus employed by M. Maisonneuve at the Hôtel-Dieu is much better adapted. Most American surgeons visiting Paris have had an opportunity of observing this method of treatment, but I do not know whether it has been introduced into this country. The moment that a limb is amputated, the stump is surrounded by a conical gutta-percha cap, whose rim fits air-tight to the skin. From the apex of this cap passes an India-rubber tube that connects with an aspirating pump. By means of the pump the liquids from the wound may be drawn off, and discharged by a second tube into a receiver. During the intervals, the elastic walls of the cap and tube coming from the wound, fall together so that no air is in contact with the suppurating surface. To dress the wound, the tube is detached from the pump, and connected with the tube of a bulb-syringe, in whose continuity is inserted a short piece of glass tubing, so that the operator may watch the stream of fluid he injects upon the stump, and be sure that no bubbles of air pass over. The detersive liquids employed are either tincture of arnica or a solution of carbolic acid. These, injected in a continuous stream on the stump, are allowed to flow off by a secondary tube, connected with that of the bulb-syringe. In this way the wound is dressed daily, without the least exposure to the air. The apparatus may also be used in cases of accidental traumatism, as compound fracture; but here it may be less efficacious, when the dead tissues have already been for some time exposed to the air.

As far as my own observation extended, this apparatus yielded excellent results, and the principle upon which it is based seems certainly most rational. I am unable to tell why it is not adopted in other surgical wards than those of M. Maisonneuve.[1]

[1] I found, after writing the above, an account of Maisonneuve's apparatus in the fifth volume of the *Practitioner*.

REPORT OF AN ADDRESS TO THE GRADUATING CLASS OF THE WOMAN'S MEDICAL COLLEGE OF THE NEW YORK INFIRMARY[1]

WOMAN'S MEDICAL COLLEGE OF THE NEW YORK INFIRMARY. —The Commencement exercises of this College were recently held at Association Hall.

PROF. MARY C. PUTNAM, M.D., delivered an address upon the true method in Medical Education. She brought out very clearly and forcibly the principle that the knowledge which is really to be of use to the physician must be that gained by his own observation, not that taken on the authority of books or lectures. From the beginning, therefore, the student should be taught to observe, to experiment, and to think, for himself. His chemistry should be learned in the laboratory; his histology and physiology by work with the microscope and experimentation on animals; his diagnosis and therapeutics by study in dispensary and hospital of actual cases, for which, under proper supervision, he is made responsible. In this way, and in this alone, would his practical skill keep pace with his theoretical knowledge; he would really know his subject, and not simply know about it. When called to a case of emergency he would be prepared, at once, and with a just self-confidence, to bring all his faculties to bear upon it—would think with his whole body, that had been trained for this very work, and not be driven in despair to his note-book or his library. Having once learned the true method of study, he could never become a routinist, but would hail every new case as a problem for fresh investigation.

The speaker sketched a plan in which all the medical charities of our metropolis should be organized in a grand system for the

[1] Reprinted from *The Medical Record*, 1872.

purposes of instruction. The students of the various schools would be divided into small classes for clinical work, each class under the immediate charge of a clinical clerk, and the whole under a central directory; so that wherever a case occurred of interest to a special class, the notification could immediately be given and the opportunity improved.

Contrasting what ought to be done in this direction with the mode of instruction at present pursued in our schools, the Doctor characterized our lecture system as an enormous anachronism, a legacy of the times of mediæval darkness, when original scientific study was a thing unknown, and the only fountain of learning was the wisdom of the ancients; when the business of the medical teacher was to give epitomes of Hippocrates and Galen, and that of the student to make and memorize his abstracts of these epitomes. Now, fully as we realize the grave deficiencies of our colleges, we think she has here done them less than justice. Their professors do not, as a rule, content themselves with reiterating the theories of past centuries, or even of the past decade. They keep abreast of the times, and give the student the latest results of investigation, which cannot be found in his text-books, but must be sought in monographs and journals. True, there are exceptions, even in our own city; but we claim that they *are* exceptions to the rule. For this purpose, then—to announce the last revision of scientific theory, as well as the newest facts of observation, and, moreover, to present the grand outlines of a subject in the vivid and impressive way which no book can imitate—we hold that the accomplished lecturer has still a most important place to fill. We agree, however, that every fact must be made the student's own by practical experiment where this is possible; that he must be taught to test every theory in the crucible of his own reason; and that the lectures are valuable only as they lead him to do so.

The only reference to the recent change of curriculum in the Medical Department of Harvard University was the statement that laboratory work, formerly optional, was now compulsory. We think the stand taken by this school deserved a more cordial recognition. Its compulsory laboratory work extends not only to chemistry and gross anatomy, but also to microscopy and physiology. Its clinical instruction really deserves the name, the senior students being expected to diagnosticate cases, prescribe

their treatment, and present written reports of them, which are discussed before a "conference" of the class, presided over by a professor. This, and other similar features, we remember as prominent in the course many years ago. Taking the above in connection with the late gradation of studies through the three years, and the required attendance on recitations during the summer, we think the school goes far toward fulfilling Dr. Putnam's idea. A preliminary examination for matriculation, and an extension of the term of professional study to four years, are still, however, great desiderata.

We have touched upon only a few points of this address, which was marked not more by its suggestiveness than by its earnestness, sometimes rising to eloquence.

ON ATROPINE[1]

A LECTURE DELIVERED AT THE WOMAN'S COLLEGE OF THE N. Y. INFIRMARY

1873

LADIES:—In inaugurating this year's course of lectures, I must first point out to you a certain change that I have made in our programme;—a change which will cause it to differ materially from that of corresponding courses delivered at other schools in this city.

Last year, imitating the system that I believe generally prevails in this country, the lectures on Materia Medica and Therapeutics were combined, and both attended by all the students, whether these had been studying one, two, or three years. On this account, students at the very outset of their career were compelled to listen to accounts of the remedial action of drugs, that their ignorance of pathology rendered completely unintelligible to them; and, on the other hand, others, preparing to graduate, were obliged to review details in the chemical and pharmaceutical history of medicines, when these, in comparison with the urgent interest offered by their therapeutical properties, could not but seem dull and unimportant. Moreover, such students, having passed through no preliminary training, were plunged immediately into one of the most complex and difficult studies in the entire range of human sciences—that of the action of drugs on the living organism in health and disease. A science that requires as basis a minute and comprehensive knowledge of physiology and pathology, was thus attacked by persons who as yet were only furnished with the most slender modicum of such

[1] Reprinted from *The Medical Record.*

knowledge. As a necessary consequence, the true complexity of the subject was ignored, and knowledge of the action of drugs resumed in a few bald formulæ, whose simplicity, no less than their rigidity, rendered them entirely insufficient as guides in the labyrinth of therapeutical problems.

As such system of instruction was illogical I have not hesitated to set it aside, though supported by so much example and precedent. This year, therefore, the course will be divided into two distinct sections. The first year students will be invited to the study of materia medica; to familiarize themselves with the properties of drugs in their natural and commercial condition; afterwards to trace them through their various pharmaceutical preparations, many of which they will have an opportunity of fabricating themselves. They will learn the origin, the botanical and chemical classification, the chemical constitution and physico-chemical properties of drugs, before attempting to rise to the contemplation of their properties in relation to vital organisms. By this means they will thus: 1st, acquire certain knowledge which otherwise they are only assumed to possess; 2d, be trained for the acquisition of other knowledge which they must in great measure miss, if they have not been previously prepared to receive it by exercise in simpler studies.

The second and third year students will study, in a two years, course, the physiological action and therapeutical application of drugs. As far as possible each proposition will be illustrated by experiments made upon animals in our laboratory, or by the treatment of patients selected from our clinics.

I will express the hope that, before long, our school will accept the standard of Europe, and create a chair of materia medica entirely distinct from the chair of therapeutics: that, further, each of the seven primitive chairs will be divided into an elementary and an advanced section. Until this is done, *all* medical education will remain elementary, and the very conception of a superior education will continue to be ignored.

. . . .

I have selected atropine as the first subject of this year's studies, because the researches that have been made in regard to it afford a complete type of those that should be pursued in regard to every reputed remedial agent. It is necessary, 1st, to observe the succession of phenomena produced by such agent after its

introduction in a healthy organism; 2d, to analyze each of these phenomena to its ultimate elements; 3d, to compare the effects of the drug upon organisms involved in various morbid conditions, with the results obtained from such analysis.

In the first place, therefore, we have to consider the general tableau constituted by the physiological effects of atropine . . .; and the first phenomenon that demands analysis is the effect of atropine on the pulse.

There is no doubt that atropine accelerates the pulse. But in regard to this acceleration we must ask the following questions:—

1st. Is this acceleration immediate or secondary? According to Harley and Meuriot, the pulse is *immediately* accelerated after the administration of atropine; this acceleration is indeed the first effect produced. According to Schroff, Posner and Nothnagel, the pulse is first lowered, and afterwards accelerated. According to Bezold, the acceleration is immediate after subcutaneous injection, secondary after ingestion of the poison. In Bezold's experiments upon guinea-pigs and dogs, an acceleration of from 14 to 48 beats frequently occurred during the first minute, or even quarter of a minute. In one dog, the pulse rose suddenly from 80 to 240 beats in a minute. In these cases the atropine had been injected into the facial or external jugular vein.

Harley's observations are less reliable, because not made until ten minutes after the injection.

In the three cases where we tested the action of atropine on human beings before your eyes, we observed a fall of the pulse within ten minutes. In the first case the patient was a delicate, lymphatic, but not nervous woman, to whom one-fiftieth gr. of sulph. atropiæ was given by the mouth, the pulse then being at 96, probably from some emotional excitement. In ten minutes the pulse had fallen to 80, and remained at 80 to the end of an hour, notwithstanding the occurrence of other symptoms of atropism, a slight flushing of the face, dryness of mouth and throat, and very slight dilatations of the pupils. In the second case the subject was a rather robust woman in good health. The pulse being at 80, one-fiftieth gr. sulph. atrop. was given by subcutaneous injection. In seven minutes the pulse had fallen to 68. In fifteen minutes came a dryness of the throat and slight giddiness. In twenty minutes the pulse had risen to 104. This rise may have occurred at fifteen minutes, as at that time the

pulse was not examined. In the third case, after hypodermic injection of $\frac{1}{64}$ grain, the pulse fell in five minutes from 92 to 80, in 10 minutes rose to 100, in 20, to 104.

You see, therefore, that both these cases contradict Bezold's statement, that the acceleration is *always* immediate after subcutaneous injection, and always secondary after ingestion; for in the cases of injection the acceleration, which was notable, was preceded by a marked diminution, and in the case of ingestion the pulse fell, and did not again rise. But in this case the subject belonged to a class that we shall find, for reasons to be hereafter noticed, is rather insusceptible to the action of atropine, and therefore the dose was too small. The other cases, on the other hand, fairly represent habitual conditions. This initial fall of the pulse is more certain to occur in human beings than in dogs, whose cardiac susceptibility to atropine is very great. It is also to be expected from subcutaneous injection rather than from an injection into veins. This phenomenon is too transitory to be of any value therapeutically, but physiologically it is extremely interesting, in connection with another atropine effect of which we shall presently speak,—I mean the contraction of the small arteries.

2d. At what doses does atropine determine an acceleration of the pulse?

On this point there is unanimous testimony. The heart's action is accelerated by *small* doses, and slackened, on the contrary, by large. In guinea-pigs, from 0.0005 to 0.02 accelerated the heart from 4 to 12 beats in 15″; while 0.05 lowered the pulse in one case from 70 to 44, in another from 80 to 58. With 0.10 the pulse first slackened, then stopped in about a minute (Bezold).[1] In the horse, with one-twelfth of a grain, there was acceleration of the pulse 10 beats in 35 minutes; with one-sixth, acceleration of 24 beats in 17 minutes; with $\frac{1}{4}$, acceleration of 56 in 12 minutes: and this was the maximum acceleration obtained. With $\frac{1}{2}$ grain it was 42 beats in 12 minutes, and with 2 grains 35 in 15, or 37 in 20. Similarly on the dog, with $\frac{1}{90}$ grain pulse rose from 120 to 300 in 14 minutes; and $\frac{1}{48}$ and $\frac{1}{24}$ grain produced the same effect; but with $\frac{1}{6}$ the pulse was 400 at the end of $1\frac{1}{2}$ hours. In man there is said to be an acceleration of 20 to 25 beats with $\frac{1}{120}$ or $\frac{1}{96}$ grain; 20–60 beats with $\frac{1}{60}$ grain, 20–70 with $\frac{1}{48}$, and

[1] *Ueber die Physiol. Wirk. des Atropins.* Leipzig, 1867.

only 30 with $\frac{1}{20}$ of a grain (Harley).[1] Meuriot noticed an acceleration of 84 beats in 90 minutes after an injection of 0.001. Nevertheless, with toxic doses, the pulse remains extremely frequent until an advanced period of the coma. In Behier's case,[2] where an old man of 75 had taken 0.013 of sulph. atropiæ, the pulse was 108 in three hours, at the beginning of profound coma, and rose afterwards and beat at 120 all night, and until return of consciousness. In a case quoted in *Amer. Jour. Med. Sciences* for 1866, from Schmid,[3] after ingestion of $\frac{1}{4}$ grain of atropia, and during period of excitement, the pulse was 130. On the other hand, in the famous case of Dr. Angelo Poma,[4] when a profound coma had set in $2\frac{1}{2}$ hours after the ingestion of f ℨ j. of solution of extract of belladonna, the pulse was extremely slow. In several other cases of poisoning it is recorded that the pulse was weak and depressible, though the number of pulsations is not given. In Lee's cases of poisoning with the analogous mydriatic, stramonium,[5] the pulse was from 100 to 150 in the two men patients, who were comatose when treatment commenced; and 140 in the woman, who was in a state of maniacal excitement resembling delirium tremens. The pulse only sinks immediately and permanently when injected into the jugular vein, a condition that evidently is never reproduced in man.

By ingestion or subcutaneous injection, and after the initial slight fall, the pulse is therefore always accelerated; and this acceleration, though not in exact proportion to variations of physiological doses, is excessive in those doses where it will ultimately or rapidly be succeeded by slackening. (See also Schroff[6] and Meuriot.[7]) This fact is important to remember, in interpreting certain details of the reactions of belladonna in cases of opium poisoning. By it we also test the value of the assertion made by Lemaitre,[8] and supported by another, quoted by him from Leusana,[9] that the effect of atropine upon the pulse is only slightly appreciable.

[1] *Old Vegetable Neurotics.* London. 1869.
[2] *Union Médicale.* 1863. [3] *Klin.*
[4] *Gaz. Hebdomadaire.* 1863. [5] *Amer. Jour. Med. Science.* 1862.
[6] Schmidt's *Jahrbücher*, Bd. 76. 1852.
[7] Meuriot. *Thèse sur la Belladone.* Paris, 1865.
[8] *Archives Générales.* 1865.
[9] *Union Médicale.* 1851.

3. How is the pressure in the arteries affected during the atropine acceleration of the pulse? Marey has formulated the following law:—"The frequency of the pulse, or of the cardiac contractions, is in inverse relation to the degree of arterial tension." It has been said, on the other hand,[1] that Ludwig and Thiry have formulated another law, precisely the reverse of this: "The frequency of the pulse increases *with* the arterial tension." In both cases the arterial tension is taken as the point of departure, and its rise or fall declared to be a *cause* of the acceleration of the heart's action. This quotation, however, is not quite incorrect. According to the exposition of Ludwig's views, made in a memoir of Bezold,[2] and also in another of Pokrawsky, after an increase of the blood-pressure, the pulse was sometimes quickened and sometimes slackened. This, whether the increased pressure was determined directly by closure of the cœliac and renal arteries, or indirectly by irritations of the spinal cord or splanchnic nerves. Thus Ranke, who admits that an acceleration of the pulse takes place when the arterial pressure is increased, observes that it occurs likewise when this is diminished, but when the force of the heart is diminished even more rapidly than the resistance in the arteries.[3]

It is evident that arterial tension may be increased, either when more blood is thrown into the arteries by greater force of the heart's action or when an obstacle exists to its efflux. An agent that, like cold, excites the active contractility of the small arteries, by accelerating the peripheric circulation, necessitates the accleration of the heart's action. The tension of the blood-vessels rises, but the rise is the consequence, and not the cause, of the quickened pulse. With any condition that weakens at once the blood-vessels and the heart, as fever, or the action of certain narcotics, the tension will be lowered, yet the heart accelerated, while the vessels are passively dilated. Both the acceleration and the lowered tension depend on the insufficiency of the cardiac contractions. With bromide of potassium the small arteries are completely constricted, the tension raised (?), and the pulse lowered. With atropine, the arteries are partially constricted, the tension raised, and the pulse accelerated. Only at the very

[1] Chauvet *De la Circulation Capillaire. Thèses de Paris.* 1869.

[2] *Untersuchungen über die Herz und Gefässnerven der Säugethiere.* 1867.

[3] *Lehrbuch der Physiologie des Menschen.* 1872.

beginning the pulse falls, and this before any effect has been produced on the small arteries. The acceleration of the pulse coincides with acceleration of the local circulation, from the increased active contractility of the arteries. As this local acceleration is sufficient to compensate the degree of obstacle caused by the constriction, the rise of tension cannot be explained by that, but by the rapidity with which the arterial system is filled.

In Meuriot's experiments upon man with atropine the line of ascent in the sphygmographic trace (percussion stroke of Mahamed[1]) remained vertical, but was not so high as normal. This would indicate that the arterial tonicity was increased, while the heart had not lost any of its vigor. At the same time, the line of descent was not separated by any appreciable interval from the upper stroke, showing that no obstacle existed to the efflux of blood into the capillaries. This rise in the tension was first noticed in 15 minutes after injection of 0.001. ($\frac{1}{100}$ gr.) and had increased in 30 minutes, the pulse quickening at the same time. In one observation, where 0.012 were injected and the trace taken in 40 minutes, the vertical up-stroke had fallen still lower, and there was moreover a rounded summit, as if with this dose and at this time, the efflux of blood was somewhat obstructed.

When the tension in the carotids was measured in dogs by a hæmometer, the pressure rose with a subcutaneous injection of from 0.005 to 0.05 sulph. atropine ($\frac{7}{100}$ to $\frac{8}{10}$ gr.). This was the limit within which the pulse rose. With injection of 0.10 ($1\frac{1}{2}$ gr.) the pulse and the arterial pressure fell together.

Similar experiments by Bezold gave similar results. In the cases already mentioned, where the dose administered caused an acceleration of the pulse, it generally caused an increase in the blood pressure also. But in one case (Guinea-pig) this sank from 92 to 72 millimetres during the injection, and did not recover its original level until 30 minutes after, although the pulse was slightly accelerated. In another, where 0.30 were injected into the facial vein of a dog, the pulse rose in $\frac{1}{4}$ minute after the injection from 60 to 192; but the pressure in the carotid sank from 140 to 20. In this curious experiment, the dog was killed by successive doses of atropine (he received in all 0.80);—artificial respiration was practised and the abdomen opened. This

[1] *Med. Times and Gaz.*, 1872.

operation generally lowers arterial tension, but in this case it rose to 30 (having sunk to 5), while the pulse beat 168 times in the minute.

From these experiments it appears, that with the moderate acceleration of the pulse during the first stage of atropine the pressure rises;—with the excessive acceleration of sudden toxic doses, of coma;—of ultimate paralysis, in a word,—the pressure sinks. We think that it may be thence inferred, that in atropinism the pulse is not accelerated because the tension is increased, but that the tension is increased because the pulse is accelerated, the heart at the same time retaining its vigor, and thus, in a given time, throwing more blood into the arteries. In the paralytic stage the heart contracts as rapidly, but with great feebleness; at the same time also there is paralytic widening of the blood-vessels, so that a double influence exists to lower the tension. These details are of special interest, in comparing atropinism with the results of section and galvanization of the pneumogastric nerve.

4th. Thus we see that the heart's action is accelerated by atropine in extremely small, *i. e.*, therapeutical doses; that this acceleration occurs immediately in dogs, after a slight initial diminution in man, and is accompanied by increased arterial tension. Upon what does this acceleration depend? The pulse is accelerated,—1st, when the muscular fibre of the heart is directly stimulated by a greater afflux of blood, itself determined by increased respiratory movements.

2d. Similar direct stimulus is felt by the intracardiac ganglia, controlling the rhythm of the heart's movements.

3d. Acceleration also occurs when the cervical sympathetic or cervical spinal cord from which it is given off is galvanized.

It is well known that the modus operandi of this influence is the subject of a famous dispute, to which we have already made allusion. According to Bezold and Pokrawsky,[1] the influence is direct, and galvanization of the nerve acts immediately upon the muscular fibre of the heart to which it is distributed. According to Ludwig and Thiry, the influence is indirect, and dependent upon variations in arterial tension. Galvanization of the sympathetic or of the cervical cord causes the contraction of whole

[1] *Ueber das Wesen der Kohlenoxyd Vergiftung.*—Dubois und Reichert's Archiv, 1866.

territories of blood-vessels, even those of the mesentery. This still occurs, when the cord is galvanized, after all the nerves going from it to the heart have been cut, and according to Ludwig, in that case the pulse is still accelerated. But Bezold and Pokrawsky affirm, on the contrary, that in this case the acceleration of the pulse is *much less marked* than when the nerves are intact. The contraction of the blood-vessels still occurs, but the direct stimulation of the heart is wanting. The three observers, however, it is seen, agree in ascribing a certain amount of acceleration of the pulse purely to the rise of arterial tension determined by constriction of the blood-vessels—contrary to the theory of Marey. According to Bezold's theory, if atropine stimulated the sympathetic in the heart, as it does in the small arteries, the heart would be directly accelerated, by stimulation of its accelerating nerves. According to the other theory, any stimulation of the cervical or cardiac sympathetic would merely reinforce that directly exercised upon the blood-vessels by the local contact of atropine. The only way to prove a direct influence upon the sympathetic is to isolate the heart by a section of the pneumogastric, and then administer the atropine. But the acceleration of the pulse after this operation is already so great, that such acceleration as might be produced by stimulation of the sympathetic would be entirely masked. It is certain that when atropine is injected after section of the pneumogastric, the acceleration of the pulse is not further increased.

The constriction of the small arteries sometimes coincides with an accelerated, sometimes with a slackened pulse. Sometimes, as in Ludwig's experiment, where the cervical cord is galvanized after section of the sympathetic nerves going to the heart, this constriction seems to be the only cause of the acceleration of the heart's action, and the acceleration is not very marked. Sometimes, as after administration of bromide of potassium, the small vessels are strongly contracted, but at the same time, the pulse falls. The same coincidence is shown in an observation of Pokrawsky's. When carbonic oxide gas was injected into the veins or inhaled, the small arteries, stimulated by blood deficient in oxygen and too rich in carbonic acid, contracted: at the same time, the pulse and tension fell "from coincident irritation of the medulla and vagus."

But the active partial contraction of the small arteries deter-

mined by atropine, with increased local circulation, is quite different from the complete contraction caused by bromide of potassium or carbonic oxide or carbonic acid gas. If the blood flows more rapidly at the periphery, the heart must contract more rapidly. Hence in this way the stimulation of the sympathetic produced by atropine would be one cause of the acceleration of the pulse. Whether there is also a direct stimulation of the fibres going to the heart we cannot consider at present as determined.

4th. The most powerful means of acceleration of the heart's action is well known to be section or paralysis of the pneumogastric nerve; after this operation the pulse rises immediately to double and quadruple its previous speed. Now, the remarkable acceleration of the pulse that follows the injection of atropine can only be compared to that determined by section of the pneumogastric. It is also most noticeable in those animals, as dogs, upon whom section of the vagi produces the most marked effect on the pulse. The tension rises after atropine, as after section of the pneumogastric. As already noticed, if the pneumogastric be severed previously to the administration of atropine, the acceleration of the pulse is not further increased, as if the agent upon which the atropine usually acted had been suppressed by the operation. Finally, if the vagus be cut in an animal previously atropinized, galvanization of its peripheral extremity will no longer produce cardiac tetanus. The electrical excitability of the sympathetic remains intact. From these facts, we think the inference[1] is indeed justified, that atropine accelerates the heart's action, by partially paralyzing the pneumogastric nerve.

At the same dose, however, atropine, as we have seen, has no appreciable effect on the respiration. Hence the main trunk of the pneumogastric nerve cannot be paralyzed, for in that case the respiration would be interfered with in the ordinary manner. Moreover, when the atropine is injected into the carotid, and sent towards the brain, the pulse is at first slackened, until time enough has elapsed for the poison to be distributed throughout the body, and reach the heart. But if it be injected in the jugular

[1] Meuriot, Bezold, Botkin, loci cit. Conclusion contested by Harley; but it is difficult to see on what grounds. Huseman, on the contrary, indorses this view.

vein, the acceleration is immediate, and much more marked than by ordinary subcutaneous injection.

Hence we may infer that the atropine acts upon the cardiac peripheric extremities of the pneumogastric nerve, partially paralyzing them; that this is the first cause of the acceleration of the heart's action. A second is the stimulation of the sympathetic nerve, possibly in the heart, but certainly in the small blood-vessels. The circulation in them is more rapid, blood passes more rapidly to and through the heart, hence directly stimulated to increased activity. This double mechanism is the first in which the action of atropine resembles that of fever. Other similar coincidences are the slight rise of temperature, the slight increase in the excretion of urea, and the diminution of secretions to which we have called your attention.[1]

. . . .

In speaking of the anæsthetic properties of atropine, it is necessary to recall Botkin's experiments,[2] and the inference that this observer draws from them, namely, that atropine primarily paralyzes the motor nerves. These experiments were made upon frogs, and with overwhelmingly large doses, and the influence of diffusion was not taken into account. In order to exclude this cause of error, the vessels and nerves of the frog's leg must be isolated, and a ligature passed round the soft parts so tightly, that the poison, injected under the skin of the other limb, cannot diffuse through the cellular tissue, but can only reach the nerve by the artery. In a limb so prepared, after administration of atropine, the sensibility is diminished, the motility remains intact, while on the other leg, where diffusion has taken place, both are diminished equally.[3] If the ligature embrace not only the cellular tissue, but also the artery, no effect on the nerve will follow the injection of atropine. This shows that the poison anæsthetizes the periphery of the nerves, and not their roots or the nervous centres. For in this second case, as in the others, it has full access to these parts, but, by ligature of the artery, fails to reach the periphery of the nerve. No anæsthesia results, though this supervenes as soon as the circulation in the limb is restored.

[1] In a part of the lecture not deemed necessary to quote.

[2] Ardrio, Virchow, Bd. 24.

[3] Result of personal experiment.

The peripheric localization of the action of atropine is shown also by another fact. In a limb under the influence of atropine, an electrical current directed to the skin (or the extreme periphery of the sensitive nerves) causes no sensation. If the same current be directed to the trunk of the sciatic, evidence of pain is obtained. Moreover, reflex contractions occur, first in the same leg, afterwards in the opposite limb, showing that the conductibility of the centripetal fibres is intact.

The motor nerves are only indirectly affected; that is, after diminution of the sensibility, there is necessarily diminution of reflex contractions. But at this time, *direct* irritation of the motor trunks produces as strong contractions as in a normal condition.

That the spinal cord is not primitively affected seems shown by the following experiment:—A ligature is passed tightly around the body of a frog, so as to separate the anterior from the posterior limbs. Atropine is then injected under the skin of the anterior half. At first, irritation of any part of the body produces reflex contractions; but as the sensibility of the nerves in the anterior half diminishes, irritation in *this* region remains without response. But irritation of the posterior half still produced contractions in the four limbs. This shows that the excited motor power of the cord has remained intact, for it is the only medium of communication between the part irritated and the parts set in motion.

Upon the voluntary muscular fibre, atropine has even less influence than upon its motor nerves. With the unstriped muscular fibre however, it is different. We have seen that by stimulating vaso-motor nerves, atropine stimulates the contractility of the muscular fibre in the arteries. According to Meuriot and Orrimus, in a rabbit killed after administration of a non-toxic dose of atropine, the movements of the intestine, always observed when the abdomen is opened, are exaggerated, and they infer that the muscular fibre is excited by the atropine. Fleming has come to a similar conclusion, from studying the action of atropine on worms. Bezold has observed, on the contrary, that the intestine of the rabbit remains perfectly still, but this is only after very large doses.

In regard to the unstriped muscular fibre of the intestine, as in striped muscle, it is necessary to separate the action of atropine upon muscular fibre from that upon the nerves, and

upon two kinds of nerves, the ganglionic and the splanchnic. In this connection Keuchel's experiments are very interesting.[1] Two cats were selected, similar in size, and in both the splanchnic nerves were cut before their junction with the solar plexus, and below the diaphragm. Then one of the cats was poisoned with 0.005 of atropine. Both animals were then killed, and on opening the abdomen immediately after death, the peristaltic movements of the intestine were observed to be increased. If now, on the cat that had received no atropine, an electrical current were sent through the splanchnic nerve, this peristaltic action is immediately arrested, just as the heart's action is arrested by galvanization of the vagus.[2] But in the cat that had received atropine, the electrical excitation of the splanchnic produced no effect whatever; the movements continued. The atropine therefore seemed to act on the intestine by stimulating the ganglionic nerves and disseminated ganglia, which provide for the dilatation of vessels; and by paralyzing the splanchnic nerves, which, by tending to contract the blood-vessels, tend to restrain and tonify the contractility of muscular fibre. The opposition is analogous to that between the pneumogastric and the sympathetic in the heart, and the action of atropine is similar in the two cases.

Precise knowledge of the mode of action of atropine upon muscular fibre is especially important for arriving at a true theory of its action on rigid sphincters, where it has been so often employed therapeutically. The usual expression, "Belladonna *relaxes* the sphincters," is extremely vague, and conveys several ideas more or less false, among others, that of *paralysis* of the muscular fibre. I would venture to suggest that a sphincter grown rigid under irritation, *e. g.*, an os uteri during parturition, is, properly speaking, *tetanized*. It is a remarkable fact that tetanic contractions are always the result of a peripheric and consequently reflex irritation,[3] as is well known clinically, and

[1] Schmidt's *Jahrbücher*, Bd. 143.

[2] As galvanization of the splanchnic is known to contract the blood-vessels, and as the exaggerated movement of the intestine is known to be due to the rapid formation of carbonic acid, when the abdominal vessels are exposed to the air, it seems probable that the movements are arrested because the supply of blood to the muscular fibre is suddenly diminished.

[3] Traumatic tetanus, of course. Strychnia tetanus seems to be of a different nature.

may be demonstrated experimentally. In a rabbit upon whom I had made a hemisection of the spinal cord, irritation of the limb on the side opposed to the section determined tetanic contractions in the limb on the same side. But direct irritation of the gray substance of the cord determined *clonic* convulsions in the adjoining muscles and in the limb.

Tetanus is not analogous to normal contractions,[1] but to cadaveric rigidity, which occurs earlier in tetanized muscles than others. In this state the fibre is shortened and broadened, and as it loses its power to contract in proportion to its shortening,[2] really remains passive and motionless, molecular nutrition is arrested, and the coagulation of undecomposed myosine around the fibre is considered by some observers, in tetanus as in permanent death, to concur at least with change of electrical conditions in the preservation of immobility. The whole chain of sequences is broken when the initial irritation is destroyed. The anæsthesia of the peripheric nerves determined by belladonna allays this irritation, arrests the transmission of exaggerated impressions to the spinal cord, and hence the overwhelming motor excitation that had been sent from it. At the same time, by quickening the local circulation the atropine may facilitate the removal of coagulated or waste substances clogging up the substance of the muscle.

. . . .

In regard to the mechanism of the mydriasis determined by atropine, you will still frequently hear it ascribed to a "stimulation of the dilating, radiating fibres of the iris." Even Stellwag, after adducing a great many facts that speak in favor of another theory, concludes by ascribing to atropine a double function. On the one hand it paralyzes the motor ocular nerve, but on the other it "stimulates the nerves distributed to the muscular fibre in the dilator papillæ,—and also in the coats of the blood-vessels."[3] The existence of the dilator is accepted on the authority of Koelliker, Valentin, Merkel; the distribution to it of the

[1] This fact corresponds to that observed by Legros and Onimus, where direct irritation of certain parts of the gray substance increased the convulsive movements in choreic dogs. Also with the clonic convulsions of epilepsy.

[2] Brown Séquard, *Journal de Phys.*, 1859.

[3] Stellwag. *Der Intraoc. Druck.* Wien, 1868. P. 93.

sympathetic nerve is inferred from the effects upon the pupil of section or irritation of that nerve.

You know that when this section is made the pupil instantly contracts, and when the peripheric extremity of the severed nerve is galvanized, the pupil dilates again. The contraction of the pupil after section of the sympathetic is supposed to result from paralysis of these fibres, and to the exclusive predominance of the circular fibres controlled by the motor oculi.

Galvanization of the cervical cord produces as much dilatation of the pupil as if the current were directed to the nerve itself. Now galvanization of the cord which is everywhere followed by contraction of blood-vessels, contracts the blood-vessels in the iris as well.[1] On the other hand, paralysis of the vaso-motor nerves from section of the sympathetic dilatation of blood-vessels in the iris, as in the head where the temperature rises, is followed by visible enlargement of its tissue and diminution of the pupil.

Stellwag insists on many facts that show a constant association between modification of the vascular tissue of the iris, and changes in the diameter of the pupil. Besides the results of galvanization of the cervical cord and of the sympathetic quoted above, he observes that mydriasis is always accompanied by a tumefaction of the ciliary processes, whose size diminishes during myasis. In the first case blood is passed out from the iris, in the second case, it flows back again to it. Ligature of the common carotid is followed immediately by contraction of the pupil,—an effect of the irritation of the brain from sudden anemia. But the secondary result on the eye is dilatation of the pupil, when the irritative effect has passed away, and the vascular tissue of the iris finds itself emptied. "Did not such mighty authorities speak in favor of a special dilator of the pupil, we should be inclined to believe that the sympathetic was distributed to the walls of blood-vessels only, and that variations in the size of the pupil were due exclusively to variations in their diameter." (Loc. cit. p. 79.)

The *suddenness* with which the pupil contracts after section of the sympathetic shows that an effect of irritation precedes the paralysis of the vessels, which occurs more gradually, though still rapidly. This sudden contraction is due to a reflex irritation of the encephalon, propagated thither by the central extremity

[1] Stellwag. Loc. cit. p. 76.

of the sympathetic. It is analogous to that which may be determined by any irritation of the brain, especially of the tubercula anadrigemina, or crura cerebelli, or by the ligature of the carotid.

The contraction of the pupil is not determined by opposite but by different influences from that which causes its dilatation; it is to be expected, therefore, that it should be expected by a different apparatus. In all the active physiological functions of the iris, the pupil contracts. For no purposes of vision does it dilate actively; in obscurity, or in vision of distant objects, the dilatation is caused by simple relaxation of the muscular fibre of a sphincter, from which the normal stimulus had been withdrawn. It is in these cases moderate, and not to be compared to the widening determined by atropine, or by galvanization of the cervical sympathetic, an operation which always constricts the blood-vessels, but only occasionally affects the retina. Any such effect that is produced is irritative, and manifested by flashes of light; hence if the contractility of the iris as a muscular membrane were called into play at all, it should be to diminish the pupil, as it does physiologically whenever the retina is irritated. But the reverse occurs—already a proof that the dilatation does not depend on the muscular elements immediately connected with the physiological functions of the iris, but rather upon its blood-vessels.

Gruenhagen, with less respect for "weighty authorities" than is manifested by Stellwag, entirely denies the existence of muscular dilating fibres in the iris.

> "The dilating muscle has never been found, only inferred, from a supposed physiological necessity. . . . The only fibres that can be isolated from the circumference of the iris are branching fibrillæ, destitute of nuclei, or covered with nuclei evidently belonging to epithelium,—while the fibres of the sphincter are easily separable, ribbon-shaped, and nucleated." . . .[1]

"The arcades, described by Koelliker, are only blood-vessels, as may be perfectly demonstrated by preparations of injected specimens."[2] "The radiating fibres that immediately surround

[1] *Zeitschrift für Rationelle Medicin*. 1866. Bd. 28, p. 180.

[2] *Ibid.*, p. 184. Through the kindness of Dr. Knapp, I have been able to myself observe these vessels of the iris, with walls whose diameter is at least half that of their cavity.

the sphincter, and which even Koelliker could not trace to the circumference, are merely dependencies of the sphincter: those beyond are elastic tissue."[1]

In a word, there is only one kind of contraction of the muscular fibre of the iris, that which contracts the pupil, in obedience to a stimulus derived from the retina or brain, and conveyed by the cerebral nerve, or motor oculi.[2] Dilatation of the pupil is never active, but, according to its degree, depends on one of three different causes. 1st, simple relaxation of muscular tonicity, or from absence of stimulus. 2d, contraction of the blood-vessels, from irritation of the sympathetic. 3d, paralysis of the motor oculi; with complete abolition of muscular tonus, and substitution of the retractility of the elastic fibres. From these considerations we may more clearly understand the mechanism of the action of atropine on the pupil.

Harley performed the following experiments to ascertain if atropine affected the sympathetic:—[3]

In the first case the sympathetic was cut, and after the pupil had contracted, atropine was instilled into the eye. Thereupon the pupil dilated, but only partially, not so much as when the sympathetic remained intact. In the second case the dilatation of the pupil was first effected by atropine, and then the nerve was cut.

The dilatation remained unchanged.

This shows that the influence of atropine upon the sympathetic in the phenomenon, though real, is subordinate.

Again, Kuyper[4] found that when the pupil had been moderately dilated by atropine, excitation of the superior sympathetic ganglion increased the dilatation.

This implies that the atropine acts upon another element in the iris than the sympathetic. The three experiments together show clearly indeed that the atropine acts by paralyzing the motor oculi nerve. For the iris, while under its influence, cannot contract, even though submitted to the reflex irritation caused by section of the sympathetic. The contracting force is

[1] *Archiv. von Pflüger.* 1870, p. 287.

[2] The extremest contraction, as after opium poisoning, is connected with passive turgescence of the blood-vessels of the iris from paralysis.

[3] *Edin. Med. and Surg. Journal*, 1857.

[4] Quoted by Béclard, *Traité de Physiol.*, 1866.

annihilated, and this force lies exclusively in the motor oculi. On the other hand, when the sympathetic has been previously divided, the dilatation caused by atropine is less, for two reasons: 1st. The motor oculi nerve is in a state of reflex irritation, and consequently more resistant to paralyzing influences. 2d. The vessels of the iris are dilated, and its tissue turgescent.

Finally, in Kuyper's experiment, the fact that irritation of the sympathetic increased the dilatation already determined by atropine, shows that the operation and the poison have acted upon two different elements, so that their effects can be superposed.

It is only at the beginning of atropinism, however, that the mydriasis is moderate, and hence resembles that which occurs after paralysis or section of the motor oculi. As is well known, the dilatation continually increases until, in extreme cases, the iris is reduced to a mere rim. This excessive dilatation cannot be ascribed to the constriction of the blood-vessels under the influence of atropine, for although that must necessarily take place, in the iris as elsewhere, it is only partial,—is an initial phenomenon, and its effects would be confounded with those of commencing paralysis of the motor oculi. It can only be due to such complete abolition of muscular tonus as must result from the paralysis of the muscular nerve of the iris, the motor oculi. The retractility of the elastic fibres then comes into play, and reduces the size of the iris to its minimum.[1]

The action of atropine in relation to the two nervous systems present in the iris, cerebral, and sympathetic or spinal, is thus quite analogous to its action in the heart. It completely paralyzes the cerebral nerve, and moderately stimulates the sympathetic.

Another analogy is revealed by the researches of Keuchel on the submaxillary gland. It is known that irritation of the chorda tympani, which, as branch of the facial, represents the cerebral influence on the gland, increases its secretion; whereas, irritation of the sympathetic, by determining a contraction of the blood-

[1] A familiar example of the effect of the retractility of elastic fibre upon inert muscular fibre may be strikingly seen in the retraction of the uterus of primiparæ after an artificial labor, with complete absence of uterine contractions, yet followed by no hemorrhage; but, on the contrary, the formation of the "*globe rassurant.*"

vessels, diminishes the secretion.[1] After injection of atropine, irritation of the chorda tympani was without effect, and a canula inserted into the duct remained dry and empty. The condition was the same as if the chorda tympani had been paralyzed, or the sympathetic irritated, and there is reason to believe that both effects had been produced.

In the iris, heart and submaxillary gland, therefore, the action of atropine is uniform—it paralyzes the peripheric extremity of the cerebral nerves, and, by stimulating the sympathetic, determines contraction of blood-vessels and acceleration of the local circulation; hence a double and analogous mechanism by which it dilates the iris, accelerates the heart's action, and diminishes the secretion in the submaxillary gland.[2]

. . . .

Therapeutical Applications.

Besides those already well known, upon which we have insisted careful study of the physiological action of atropine is continually leading to new applications in therapeutics. I have spoken to you of the suggestion made by Harley, in regard to the use of atropine as a diuretic, and as especially adapted for the treatment of albuminuria. I have had no opportunity to test this suggestion, and do not know whether it has been tried by other physicians than Harley. Upon another theoretical deduction I will however insist, as I have begun to collect some practical evidence in its favor. The dilatation of the cerebral blood-vessels that occurs as a secondary effect of atropine,[3] suggests the utility of this substance in functional cerebral anæmia. One case in which I tried atropine was that of a woman, who, three weeks

[1] And which is succeeded, during the rigor mortis, by a moderate contraction of the pupil.

[2] Keuchel's experiments on the splanchnic nerves above quoted, would seem to show an exception to the general action of atropine on the sympathetic. I do not know whether they have been confirmed; I have not yet had an opportunity of verifying them myself.

[3] On sacrificing a rabbit forty-eight hours after administration of large, but not toxic doses of atropine, the pia-mater of the brain and cord were found engorged with blood, and the arteries of the cord dilated. The initial constriction of the blood-vessels in the nerve-centers, upon which, we believe, M. Brown-Séquard bases the employment of belladonna in epilepsy, is not in contradiction with this equally indubitable fact.

after confinement, and being then in a debilitated condition, fell down a flight of stairs. She remained insensible for two hours, and for two days was unable to walk, although she had received no external injury but a bruise on the shoulder. On the fifth day she was still so giddy that she would fall unless she supported herself as she walked, and suffered from continual nausea, general muscular weakness, and occasional blurring and blackening of vision—all persistent effects of the cerebral concussion. I ordered 1-64th of a grain of atropine in solution, three times a day. The patient felt a sensible improvement in strength after each dose, as soon as its physiological effects, flushing of the face, increased dizziness, and a certain mental apprehensiveness, had passed away. On the following day, the vertigo and staggering had quite gone. In the second case, the diagnosis was more obscure. The patient presented herself at the Dispensary for Nervous Diseases, complaining of vertigo, general muscular debility, and especially paresis of the right arm, without any trembling. There was a faint blowing murmur at the apex of the heart.[1] Atropine was given, as an experiment, to try the effect upon the vertigo, 1-64th of a grain, at first three times, afterwards twice a day. The physiological effects, as in the first case, were extremely well marked—the flushing of the face intense, and lasted an hour. Under this treatment the vertigo entirely disappeared, and the patient gained in strength. The treatment was afterwards complicated with nutritive tonics and electricity. In a third case of cerebral and general anæmia, without any sign of local cerebral lesion, but with vertigo and floating specks before the eyes, the vertigo quite disappeared under the exclusive use of belladonna extract. The other anæmic symptoms were only relieved by blood tonics.

. . . .

Finally, there remains for us to say a few words on the so-called antagonism existing between belladonna and opium, in cases of poisoning. Let us notice, in the first place, that the real antagonism conceivable is not that between opium and belladonna, but between some of the effects produced by the one in the living organism, and those determined by the other. Now, there

[1] About six months afterwards this patient developed marked symptoms of *paralysis agitans*.

are no two substances whose entire series of physiological effects are directly opposed to each other. There are no true antidotes to poisons but such agents as effect a chemical alteration of the toxic substance, and opium and belladonna have no such mutual reaction. Moreover, it is certain that the physiological effects of the two drugs are not in all points contrasted. Mitchell,[1] Eulenberg,[2] and Harley[3] have shown, both in experiments upon animals and in observations upon man, that opium and belladonna, taken in succession, caused greater acceleration of the pulse than the belladonna alone; also that when sleep has been induced by a therapeutic dose of morphine, atropine will not disturb, but rather render it more profound. (Harley.) The anæsthesia, diminution of secretions, dysuria, produced by one of these substances, are determined by the other also, and cannot therefore be antagonized. But, on the other hand, the pupils contracted by morphine were seen to widen by atropine, or the reverse, and the respiration, slackened by morphine, to be slightly accelerated by atropine. (Erlenmeyer.) Again, though morphine prove unable to slacken a pulse accelerated by atropine, the fact that atropine accelerates a pulse that has been slackened by morphine is of the highest importance in toxicology. This may be seen even in the rabbit, although this animal is much more susceptible to morphine than to atropine. In a rabbit to whom I had given hypodermically three grains of morphine in the course of an hour, the pulse was 148, respiration twenty-four, pupils moderately contracted, animal in partial stupor, but not insensible. Three-quarters grain of atropine were injected, and in ten minutes the pulse had risen to 240, the respiration to thirty-two, though the pupils were not yet dilated. After injection of a grain and a half more, the ears became very hot, with marked dilatation of the arteries. This subsided again after the injection of one-half grain of morphine. On further injection, in divided doses of five grains of morphine, the pulse became very weak, but numbered about 200. Injections of five grs. atropine then restored dilatation of auricular arteries, and caused full dilatation of the pupils, one and three-quarter hours from the time of its first administration. The animal recovered completely. The effects upon the rabbit's ears were particularly noteworthy in this case.

[1] *Am. Journal Med. Sciences*, 1865.

[2] *Bulletin de Thérap.*, 1867.

[3] Old Vegetable Narcotics.

It corresponds to that observed on the ears of guinea-pigs by Wegner,[1] and to the well-known experiments of Wharton Jones on the frog's foot with solution of atropine and Battley's solution of opium. The attempt to prove or disprove an "antagonism" between opium and belladonna frequently confuses the perception of the real questions, viz.: What physiological effects of belladonna are theoretically useful in the morbid state induced by opium? and further, to what extent do recorded cases of poisoning show that these effects have been produced? How have others been modified? Finally, what explanation can be given of the toleration shown by many patients for one poison, while they are already under the influence of the other?

This statement of the case is so simple as to almost seem superfluous, were it not evidently so often overlooked. In the experiments of Camus, so often quoted, full toxic doses of morphine and atropine were given almost simultaneously, and before the effects of morphine had had time to become manifest; in other words, before the conditions of resistance to the one poison had been developed by the other. It was to be expected, therefore, that the animal should feel the full force of both, and succumb.

For the morphine and atropine, even when acting upon the same organ, and in an opposite manner, affect different parts of its apparatus. Thus, atropine accelerates the pulse by paralyzing the peripheric extremity of the pneumogastric; morphine slackens the pulse by increasing cerebral pressure and the tonus of the central end of the pneumogastric. It is therefore easy to understand why atropine should accelerate the pulse in spite of morphine, while morphine should be unable to reduce an atropine acceleration. When the cardiac end of the pneumogastric is paralyzed, it avails little that the tonicity of the central end be increased. And when this has been increased by the opium congestion of the encephalon, the effect on the pulse is nullified so soon as the connection between the heart and brain is severed by paralysis of the pneumogastric. Again, the contraction of the pupil, which occurs after irritation of nearly all the organs of the encephalon, is also determined by the cerebral congestion of opium. While, to produce dilatation, the atropine acts on the periphery, on the iris itself, paralyzing the motor oculi nerve, and so cutting it off from the brain, and moreover contracting its

[1] Quoted by Stellwag. *Der. Intraoc. Druck.* P. 61. Wien, 1868.

blood-vessels by stimulus of the sympathetic. It is evident that both these effects on the iris might be produced, although no change had taken place in the condition of the brain, and hence dilatation of the pupil may occur after administration of atropine in opium poisoning, yet the patient remain narcotized, and finally succumb. Thus, in Blake's case,[1] the child, who had swallowed a teaspoonful of laudanum during convalescence from pneumonia, was treated with eighteen drops of fluid extract of belladonna in divided doses. The pupils began to dilate after the second hour, but other symptoms were aggravated, and the patient died in thirteen hours. Here the effect of the opium was much intensified by the pulmonary disease, and the dose of belladonna was small. In one of Norris' cases at the Pennsylvania Hospital, a man of 55 years, who had taken an ounce of laudanum, was treated nine hours afterwards with eight and a half grains of ext. bellad. in divided doses during three hours. At the end of this time the pupils dilated, but the general condition remained the same, the *pulse almost insensible*, and the patient died three hours later.

Again, there are cases where the dilatation of the pupils did not occur until after enormous doses of atropine have been taken, had manifested their influence in other ways, especially by the acceleration of the pulse, and been followed by a commencement of convalescence. Here the paralytic turgescence of the blood-vessels of the iris persisted after other symptoms. Thus, in one of Blondeau's cases,[2] a teaspoonful of laudanum had been swallowed, and occasioned drowsiness, coldness of extremities, contracted pupils, but no coma. A fluid drachm of tincture of belladonna was given in divided doses, and the pulse and temperature rose under its influence, but the pupils remained very contracted until some time after convalescence had evidently set in.

In Duncan's case,[3] two ounces of laudanum had been taken, and the patient was in a profound coma when the belladonna was given. This persisted after administration of an ounce of tinct. belladonna in divided doses, and the pupils continued to contract more and more. Then fifteen grains of extract bellad. were given by the rectum, and two hours afterwards the pulse rose, and the respiration became freer. Then two grains more

[1] *Archives Gen.*, 1864. Quoted from *Pacific Journal*.

[2] *Ibid.*, 1865.

[3] *Archives Gen.*, 1864. *Am. Med. Journ.*, 1862.

of the extract were given, and thereupon symptoms of belladonna intoxication occurred, with, for the first time, dilatation of the pupils. Thus the turning-point in the narcotism, as manifested by the effect on the pulse and respiration, was reached before the pupils were moved, but the return of consciousness was delayed until the moment of their dilatation.

In the remarkable case related by Constantin Paul,[1] where the injection of an ounce of laudanum had thrown the patient into a state of intense excitement instead of coma, but accompanied by great contraction of the pupil, a large part of the laudanum had been rejected by vomiting before any belladonna was given, and the recovery would probably have taken place without any medication. But the immediate effects of the belladonna upon the symptoms were none the less striking. Twelve drops of tinct. bellad. were given every hour, and in ten minutes after each dose there was marked amelioration of the vertigo and violent nausea, though the pupils remained contracted. The opium symptoms returned in from one-half to three-quarters of an hour, to disappear again with a fresh dose of belladonna.

By narrowing the interval between the doses, the convalescence was definitely established; but not until the patient had taken over f. ℥ ij of the tincture did pallor of the face and dryness of the mouth appear, while the pupils only dilated after ingestion of nearly f ℥ iv. The effect of opium upon the dilatation of the pupils, in cases of belladonna poisoning, is more difficult to appreciate because it is omitted from many of the histories. In Lee's,[2] however, where a child of 6 years old, poisoned with belladonna, was treated with 120 drops of laudanum, it is said that the purple flush began to fade from the face "*as* the pupils contracted." In a case recorded in the *Dublin Medical Press* for 1864, the pupils, dilated and motionless after $\frac{1}{2}$ grain of atropine, began to contract under the administration of opium that had been preceded by an emetic; at the same time the eruption began to fade. After 4 grains of opium had been taken the pupils were normal, and the patient (who was only $2\frac{1}{2}$ years old) fell quietly asleep.

But on the other hand, in a case quoted in the *Union Médicale*, 1863, where from 10–15 drops of laudanum were given to a child 26 months old, poisoned by an unknown quantity of belladonna,

[1] *Bulletin de Thérapeutique*, 1867. [2] *Am. Journ. Med. Sci.*, 1862.

the pupils did not contract until some time after the patient had fallen asleep, and convulsive movements had ceased. The age of the patient in this case renders this fact all the more remarkable.

Hence observation of facts justifies the expectation of theory, that in cases of poisoning by one of the two substances, opium or belladonna, the therapeutic influence of the other cannot be tested by the state of the pupils, although it is in their movements that the physiological opposition of opium and belladonna is most manifest. The pulse affords a much better test. In all cases of laudanum poisoning with a slow pulse that have recovered under the administration of belladonna, the pulse has risen in frequency and strength, and we have already pointed out several cases where this rise marked the entrance upon convalescence, and preceded, by a considerable interval, the dilatation of the pupil. In an observation in the *Bulletin de Thérapeutique*, 1865, the patient had taken 5 drachms of laudanum, and the belladonna was not given until 24 hours afterwards. At this time the effects of the poison were already attenuated, but there remained frequent vomiting, the pupils were contracted and the pulse 52. After hypodermic injection of 10 drops of a solution of atropine at 1 per cent., the vomiting instantly ceased, and the pulse rose to 68.

In another case in the *Lancet*, 1869, the patient was comatose, with stertorous breathing. The pulse was not counted until after the administration of $\frac{1}{4}$ grain of atropine, but it was then found at 160, and at the same instant the pupils dilated widely.

In one of Blondeau's cases,[1] ℥ ijss of laudanum had thrown the patient into stupor, but not coma, and the extremities were cold, the pulse small, slow, and intermitting. After the administration of f. ℥ j of tinct. belladonna, in doses of 10 and 5 drops the pulse rose, the extremities became warm, and convalescence progressed from this moment.[2]

In Anderson's case, *Edin. Monthly*, 1854, profound coma had set in, after ingestion of 9 grains of morphine, taken for delirium

[1] *Gaz. Hebd.*, 1865. This case already mentioned, in speaking of the dilatation of the pupil.

[2] This case is rejected by Harley, because the dose of laudanum was not excessive, and patient might have recovered spontaneously. Nevertheless it *does* show perfectly the mode of action of belladonna upon opiate symptoms, when these are not too intense to be modified.

tremens during a period of 36 hours. The pulse was slow and very feeble. 8 drachms of tincture of belladonna were given in divided doses, f. ʒ j every half hour, and then the pulse rose, and became strong. At the same time the coma was quite dissipated. In this case the pupils dilated after the 3d dose, before any really favorable symptom was manifest, but the pulse, respiration, and consciousness were only affected at the ninth hour.

In Blondeau's second case (*Archives de Médecine*, 1865) 100 drops of tinct. belladonna were given in the course of an hour and a half, the patient remaining insensible, and the pupils contracted and motionless. After the last ten drops the pulse increased in force and frequency, (the pupils began to dilate at the same time). The recovery was assisted by frictions (?) of the thorax.

In McGee's case (*Am. J. Med. Sc.*, 1872) the coma, determined by 30 grains of opium, was combated by subcutaneous injections of one-fourth of a grain of sulph. atropia, in divided doses. The pulse rose to 140, and at the same time the pupils dilated and vomiting occurred, which emetics had previously failed to produce. There was afterwards some return of the opium symptoms, somnolence and contracted pupils, but these disappeared spontaneously, so that the recovery really dated from the rise of the pulse.

There is only one case on record where the rise of the pulse failed to initiate recovery. This is the remarkable case of Norris, at the Pennsylvania Hospital, where 75 grains of morphine had been swallowed, and marked somnolence had not occurred until four hours afterwards. Before this time the patient was treated with tannic acid, an emetic of sulph. zinc and ipecacuanha, a strong decoction of coffee, and 20 grains of extract of belladonna. As the coma advanced, 20 grains more of extract belladonna were given in two doses, the pupils dilated, the pulse rose from 80 to 120, but the somnolence persisted. The patient ultimately recovered under the free use of stimulants. This case much resembles that of Camus's rabbits, for the doses of morphine and belladonna were enormous, and administered nearly simultaneously, the first 20 grains of the extract having been given before the effects of the morphine were well developed. As in the experiments, therefore, the effects of the two poisons, instead of neutralizing each other, accumulated, and a belladonna coma succeeded to that induced by morphine.

In Norris's other case, already quoted, with a fatal issue, auscultation of the heart showed 120 pulsations, but these were so feeble that the pulse at the wrist was almost imperceptible. The atropine failed, therefore, to act as a cardiac stimulant.

In opium poisoning the great danger lies in the congestion of the brain. The contraction of the pupils, the coma, the slowness of pulse and respiration are of importance, as symptoms of this congestion, and the latter more especially, as initiating the mechanism of death. Any antagonist to opium that does not act as a chemical antidote in the stomach, must act by dissipating the cerebral congestion. Hence it is not easy to understand why Harley pronounces atropine useless because "it does not influence the respiration, where the action of opium is the most dangerous." The surest way to restore the respiration is to dissipate the cerebral congestion. And this may be done, when the paralysis is not too complete, by all agents that quicken the heart's action, and more especially accelerate the circulation in the brain. Now Harley himself admits that atropine "is one of the most powerful cardiac stimulants we possess,"—he points out the immediate relief to the nausea occasioned by therapeutical doses of morphine, from its depressant action on the vagus, that is afforded by small doses of atropine, which neutralize this action. It is not therefore true that the cerebral effects of morphine and atropine are not opposed to each other, and Harley's experiments, showing that the sleep induced by morphine is not disturbed, but rendered more profound by atropine, does not disprove their antagonism.

In the cases on record, the belladonna has been used when the patient was in one of two conditions: 1st, a state of restlessness and intense nausea; 2d, somnolence or complete coma. In the first case, the relief has always been immediate and striking. Thus in the first case of Behier,[1] the patient had been partially relieved by abundant spontaneous vomiting, but remained alternating between somnolence and extremely painful nausea and giddiness. One-fifth of a grain of ext. bellad. was given, and immediately these symptoms disappeared. In the case already referred to, recorded in the *Bulletin de Thérap.*, 1865, the effects of the laudanum, ingested about eighteen hours previously, were passing away, but the pulse was at 56, and there

[1] *Archives de Médicine*, 1864.

was frequent vomiting instantly checked by the injection of a very minute quantity of atropine. [See above.]

The same effect is seen in the case of Constantin Paul's, already quoted, where violent nausea and agitation constituted the main symptoms of the opium poisoning, and were strikingly relieved by belladonna. As has been said, a marked amelioration occurred ten minutes after each dose of twelve drops of the tincture,—and this amelioration was permanent so soon as the interval between the doses was shortened from one hour to three-quarters, which was done after the sixth dose. It is not therefore correct to say, as Harley does in his comments on this case, that although the belladonna was given from the second hour after the poisoning, no decided effect was produced till the fifteenth hour. It was remarkable in this case, where the opium had produced agitation and not somnolence, the first phenomenon of definite recovery was sleep.

In the second class of cases, where the patient was comatose at the time of commencing the belladonna treatment, the effect of this latter is precisely measured by its effect on the pulse. If the pulse rises, the coma begins to be dissipated, and if the coma returns, the pulse has fallen again. That the effects of atropine, like those of other stimulants, should sometimes be only temporary, and unable to overcome the opium congestion, proves no more against the usefulness of atropine than against that of coffee or brandy. That, when the two poisons have been administered simultaneously or in rapid succession, the more energetic effect of atropine on the pulse may be manifested without any corresponding modification of the cerebral symptoms merely, show that until the conditions of resistance have been developed by the action of one poison, the system is equally open to the effects of both; but that, in many cases of opium coma, belladonna has quickened the heart's action, and by so doing helped to dissipate the cerebral congestion; that, in a certain number of cases, this effect has been permanent, and even to be attributed to the belladonna alone, we think unquestionable.

Most of the recorded cases have been tabulated by Harley, and commented upon. It is worth while to pass briefly in review both the cases and the comments.

In the first three cases noticed, that of Norris,[1] Blake,[1]

[1] *American Journal*, 1862. [1] *Boston Med. and Surg. Journal*, 1864.

and one from the *Pacific Journal*, 1862, the administration of belladonna was unsuccessful. We have already noticed these cases, and pointed out that in none was the pulse perceptible *at the wrist.*[1]

Concerning the case of Anderson, already quoted, Harley remarks that the coma persisted from 10 to 14 hours after the use of belladonna. But we have seen that it *was* dissipated as soon as the pulse rose.

In Motherwell's case,[2] f. ℥ jss of laudanum had been taken and twelve hours later the patient was completely comatose. The belladonna treatment was begun the 14th hour, and the coma did not begin to pass away before the 17th. But this was as soon as could be expected, and the amelioration coincided with dilatation of the pupils. [Nothing is said about the pulse.]

Now when, as in some cases considered above, the coma has been really aggravated by belladonna, the pupils *dilate*, without other sign of amelioration.

In Mussey's case,[3] there was coma and a pulse of 50, the fifth hour after ingestion of f. ℥ j. of laudanum, which persisted in spite of vomiting induced by strong coffee at 3d hour. At 5th hour, grs. vj of ext. bellad. were taken; at 6th hour, f ℥ i tinct. bellad. and at 7th hour, coincidently with dilatation of the pupils, the pulse and temperature improved. By the 8th hour the skin was warm, pulse 100, and stupor had disappeared.

Harley overlooks these signs of improvement at the 7th and 8th hours, and insists on the fact that consciousness did not return till the 11th hour, 6 hours after administration of belladonna. According to him the coma was prolonged by the belladonna. But although the patient remained unconscious after some of the characteristic effects of belladonna were produced, the sleep lost its stupor and alarming character in two hours after the administration of the belladonna.

In Lee's case,[4] where a child of two years old was in a profound coma from laudanum, tinct. bellad., given in doses of 15 minims, produced a most decided effect. After the second dose the temperature of the skin rose; after the third the pupils became

[1] See above in regard to the cardiac pulsations.

[2] *Med. Times and Gaz.*, 1862.

[3] *Amer. Journ.*, 1862; also *Cincinnati Medical.*

[4] *Am. Journ. Med. Sc.*, 1862.

sensible to light, and the child spoke; after the fourth, the pupils suddenly dilated, the face, neck, and arms became scarlet, and the child began to laugh and cry in the first stage of atropine poisoning. Recovery was prompt. Concerning this case Harley says that the necessary details are omitted, because the quantity of laudanum was not stated. But it is unnecessary to know the exact quantity of the poison when the condition of the patient is accurately stated,—since in different individuals, and different conditions of absorption, the dose required to produce any given effect is very variable.

In Mitchell's case (*N. Y. Med. Journal*, vol. iv.), ingestion of grs. v. of sulph. morphine had not produced coma $4\frac{1}{2}$ hours afterwards. The patient still answered questions correctly. It was at this time that f. ʒ vj tinct. belladonna were given, and as in Norris's case, already discussed, the stupor continued to increase till the 10th hour. During this time f. ʒ v of the tincture were given in two doses, and $\frac{1}{8}$d gr. atropine in four doses. The first characteristic effect of belladonna appeared at the 10th hour after the first dose of atropine, when the pupils began to enlarge, and after the last dose there was a scarlet flush from head to foot, with dryness of the tongue, and soon the stupor was replaced by busy delirium.

This case is more interesting pathologically than therapeutically, for the amount of belladonna taken was relatively more poisonous than the five grains of morphine: and galvanism was administered between the sixth and tenth hours. The continual increase of the coma, during the administration of doses of belladonna themselves large enough to produce paralysis and coma, might be attributed to the cumulative effect of the two poisons, were it not for the absence of mydriasis. This always occurs in belladonna coma, and we have seen that it may occur when the system is saturated with belladonna, even though the narcotism first determined by opium persists. The pulse was already paralyzed and 170, before the atropine was given, so that no characteristic effect on it could be produced. It descended, probably under the influence of galvanism, to 150 and 140. As the atropine only dissipates the coma by quickening the pulse, it could not be expected therefore to have any direct effect upon the narcotism in this case. This case, and the analogous one of Norris (poisoning with seventy-five grains of morphine, stupor

not till four hours afterwards) can only be explained by an arrest of absorption. It is on account of such arrest, from paralysis of the nervous, muscular, and secretive apparatus of the stomach, that spontaneous recovery has occurred after enormous doses of opium. Camus has collected a few such cases in his thesis.[1] The belladonna is absorbed as gradually as the opium had been, and being eliminated rapidly by the kidneys, does not accumulate in sufficient quantity to produce its own paralytic effects. Hence two facts, observable in Mitchell's case: 1st. That the first visible effects of doses large enough to produce a coma, were those that belong not to the latter, but to the initial period of intoxication, namely, a scarlet flush and busy delirium. 2d. That a large amount of urine was passed before recovery was complete. It seems probable, and the hypothesis would be easily tested by direct experiment, that the diuresis determined by the belladonna helped to eliminate the morphine from the system. The experiments of Percy show that one-quarter grain of atropine would apparently neutralize the effects of a toxic dose of morphine, when plenty of water was allowed to the animal, and free diuresis occurred; but that death would follow when, all other circumstances remaining the same, the supply of water was cut off. Hence a second, though subordinate mode of action in which atropine may be useful in opium poisoning. In cases where its influence as a cardiac stimulant cannot be exerted, or is unavailing, it may still act as a diuretic, and favor the elimination of morphine from the system. In Lucas' case,[2] where a child of eleven had swallowed f. ℥ jss. of laudanum, and, three hours afterwards, was almost completely comatose, Harley lays great stress on the fact that in the treatment electro-magnetism was employed as well as belladonna. But this was only given in order to arouse the patient sufficiently to swallow the belladonna, and the effect of each application was most transitory. The treatment was commenced at the eighth hour, with one grain ext. belladonnæ, and this repeated six times in the course of three hours. After the fourth dose the stertor was less marked, pupils less contracted, and pulse 104. After the sixth dose the stertor quite disappeared, the face was highly flushed, the pulse at 136. The effect of the belladonna in this case seems indubitable. Here, f. ℥ jss. of laudanum, or forty-eight times the full medicinal

[1] *Thèses de Paris*, 1865. [2] *Med. Times and Gaz.*, 1865.

dose, produced coma with cold extremities and livid face in three hours, showing that absorption had fully taken place. Whereas in Mitchell's observations, just discussed, after grs. v. of morphine or only thirty times medicinal dose, we have been obliged to infer that the stomach was paralyzed by the excess, and hence absorption deferred. It is certainly difficult to explain this variable action, but the fact is incontestable,—for the patient was still able to respond to questions at four and one-half hours after the ingestion of morphine. In the absence of experiments that might easily measure the rate of absorption, hypothesis is useless, and mere guessing. It cannot be said that laudanum is always absorbed more quickly than morphine, for some of the most striking cases of delay in toxic symptoms have been observed after enormous doses of laudanum.

In Duncan's case,[1] Harley again attributes the prolongation of the coma to the enormous doses of belladonna (f.ʒj tincture, and grs. xvij extract) that were given; f. ʒij of laudanum had been swallowed, and in one and one-half hours, patient was already almost insensible to external impressions, and sunk in a comatose sleep. After the administration of the f. ʒj of tincture in a single dose (preceded by emesis), the coma persisted, and the pupils continued to contract, the skin grew cold and covered with a viscid sweat, the pulse imperceptible. Since the pupils remained contracted, it is difficult to attribute this coma to the action of the belladonna. When the pulse is accelerated without improvement of other symptoms, it may sometimes be questioned whether the diffusion of the opium is not thereby favored, and its toxic effects increased. But in this case no effect was produced on the pulse. Moreover, if f. ʒj of tincture had added to the opium paralysis, the additional administration of fifteen grains of extract should have been fatal; yet after this the pulse rose, and the respiration at the same time became freer. Two grains more were given, and an hour later the pupils dilated, regained their sensibility, the patient was roused from the coma, and replied to questions.

In Adamson's case[2] of poisoning by laudanum, f. ʒiij tinct. belladonna given in divided doses between $3\frac{3}{4}$ and 9 hours afterwards. From the 2d to 5th hour there was no improvement, but at the 7th hour the patient was sufficiently roused to answer

[1] *Am. Journ. Med. Sci.*, 1862. [2] *British Medical Journal*, 1866.

questions. Harley objects to this case that other means were used besides the belladonna; but these consisted exclusively of an emetic of sulph. zinc at about the 2d hour, which induced no vomiting; and in the removal of a little fluid by the stomach-pump.

In Cazin's case, quoted in the *Edin. Monthly*, 1855, f. ℥v. laudanum had been taken in two doses. $4\frac{3}{4}$ hours afterwards the patient could not be roused from stupor and the pupils were contracted to mere points. Tinct. belladonna f. ℥j and f. ℥ij were given between $5\frac{1}{4}$ and $5\frac{3}{4}$ hours, and at 7th hour the pulse was stronger, the pupils began to dilate, and the stupor to lessen. The improvement continued steadily to the 10th hour, which marked definite recovery.

Harley again objects that in this case electricity and emetics were also used, and that their effects complicate those of belladonna. But these means were tried about the 4th hour, and an hour afterwards, when the first dose of belladonna was given, the patient was profoundly comatose, as above described. But improvement began about an hour after last dose of belladonna.

To resume the conclusions that may be drawn from the above analysis of observations and experiments:—

1st. If very large doses of belladonna be given *before* the establishment of opium coma, still more, if given simultaneously with the opium, the paralytic effect of both poisons may be produced. [See experiments of Camus, which I have repeated with similar results—case of Norris, case of Mitchell.]

2d. When belladonna is given alone, in doses sufficient to produce coma, the pupils dilate, and the pulse is accelerated, until after the most advanced stage, when it falls. When, therefore, a coma persists in a patient who has taken both opium and belladonna, if there is dilatation of the pupils and rise of the pulse, the coma may be attributed to the accumulated effect of both poisons. But when the pulse and respiration remain slow and the pupils contracted, there is no proof that the belladonna has exerted any influence at all, and the coma must be ascribed exclusively to the effect of opium not yet counteracted by medication.

3d. It is known that after excessive doses of opium, symptoms of poisoning are often delayed longer than when smaller quantities have been taken, and the delay is attributed to tem-

porary paralysis of absorption. This same condition partly explains the impunity with which patients plunged in opium coma bear such enormous doses of belladonna. If only small quantities are absorbed of the mass contained in the stomach, while elimination is rapidly going on by the kidneys, some time might elapse before any great amount is circulating at once in the blood.

4th. The diuresis determined by atropine favors the elimination of the opium alkaloids, and in some cases recovery seems to be mainly due to this cause.

5th. The main action is however upon the circulation. The capillaries, paralyzed and distended by opium, are directly stimulated to contract by belladonna, and at the same time the heart is quickened by being released from pressure of the pneumogastric. A double influence is therefore exerted to dissipate congestions; and as cerebral congestion lessens, the respiration, so dangerously menaced, becomes freer.

6th. Hence the therapeutic value of belladonna in any given case must be calculated exclusively from its effect on the pulse and on the kidneys. The dilatation of the pupils only shows that the system is under the influence of atropine, not that that influence is beneficial. Coma may persist, and the patient die, with dilated pupils. This is the case when animals are poisoned by toxic doses of opium and belladonna given simultaneously.

7th. In therapeutic doses, the pulse, slackened by morphine, is always accelerated by atropine, and the reverse is not true. The effect of atropine on the pulse is relatively more energetic, for reasons above detailed. But it is certain that in the majority of toxic cases recorded, the acceleration of the pulse is only produced with difficulty, and then coincides with an amelioration of the narcotic symptoms. In the one or two instances where there was not such amelioration [case of Norris], the pulse, though accelerated, remained very feeble, so that no real stimulation of the circulation was produced, but only a double paralysis. There are cases where immense doses of belladonna have been swallowed at the very moment that absorption was beginning to take place, after the temporary stupor induced by opium, and before the establishment of coma had again diminished its activity.

8th. There is nothing either in theory, or in the observation of facts to necessitate or justify the enormous doses of belladonna

that have been given. It is known that the acceleration of the pulse and rise of vascular tension are produced by small doses, and the contrary effect by large. It is more rational to administer ♏xv of the tincture at intervals of a quarter or half an hour, and this treatment has been followed by more satisfactory results than the administration of f. ℥ j at a dose. It is absurd to calculate the amount of belladonna needed from the amount of opium that has been swallowed, for the neutralization required is not chemical, but physiological, and to be adapted to the reactions of the organism.

9th. The toleration of such enormous doses of an opposite poison is none the less a remarkable phenomenon in the pathology of opium poisoning. When taken simultaneously, the effects are different from those noticed when the belladonna was given some time after the opium, though before the occurrence of coma. These effects are of three kinds. 1st. No toxic symptoms may be observed. [Case of Cazin,[1] where a liniment containing f. ℥ jss. of laudanum, and f. ℥ ss. tincture of belladonna was swallowed, without other result than somnolence and dilatation of pupils.]

In this case the dose of belladonna was not excessive, there was abundant diuresis [the patient had previously had complete retention of urine], and this easily explains the elimination of morphine before any narcotism could be produced.

2d. There may be severe symptoms of poisoning, but followed by spontaneous recovery. [Case of Christison,—three successive injections containing each ℈ij of opium, and ℥ ss of belladonna leaves.] In this case there was profound coma in three hours, with dilated pupils, showing predominance of the belladonna poisoning.

3d. Coma may set in, apparently less profound than results from opium alone, but tending to a much more rapidly fatal issue. This has only been seen in experiments on animals, for in the three cases where death has followed the belladonna treatment, the doses of belladonna were much smaller than in those that recovered. (Grs. vj of extract, in Norris's case, f. ℥ ij tincture, in Blake's and only ♏ xviij in the case of the child related in the *Pacific Journal.*)

Even therefore when several hours have elapsed after the administration of belladonna, without occurrence of any per-

[1] *Traité de Plantes Médicinales.*

ceptible amelioration, and where the therapeutic efficacy of the drug might apparently be called in question, the problem of its tolerance remains to be explained. We have suggested a partial explanation in the condition of its absorption and elimination; we do not affirm that none other is possible nor needed. But the combination of properties possessed by atropine as a diuretic, a cardiac stimulant, and stimulator of the vaso-motor nerves, affords a theoretical explanation of its action that at least lies nearer to facts positively known than the hypothesis of vague "resistance." When a certain amount of morphine had been eliminated from the system by the kidneys, the atropine is then able to exercise its most important action on the circulation, and thus directly dissipate the cerebral congestion.

It is extremely important to settle this question by examination of the urine of patients comatose from opium, and to whom atropine or belladonna has been given.[1]

10th. Belladonna is no "antidote" to opium, nor even to the entire series of pathological phenomena determined by that poison. Nor is this surprising, since there are no antidotes to pathological entities which do not indeed exist. But theoretically and practically it does modify some of the phenomena of opium poisoning, and may be used to advantage within the limits of the following rules.

1st. It should not be given as a prophylactic, but only to combat conditions already existing, either of restlessness, nausea and vomiting, or of somnolence, stupor or coma.

2d. It should not be given in large doses, but in small ones [♏ xv] frequently repeated.

3d. It is safe to continue the administration so long as the pupils are not dilated nor the pulse accelerated. If dilatation has taken place, yet the iris remains motionless,—if the pulse has become rapid and weak, and coma still continues unabated,—further use of atropine would only increase the mischief.

4th. The use of adjuvants, as emetics, coffee, if necessary, electricity, is to be recommended as much in the belladonna treatment as in any other. The previson of the physiological effects to be expected from belladonna enable us generally to analyze its influence, even when a complex medication has been employed.

[1] This elimination in substance of atropine with the urine is known to be the cause of the dysuria that so frequently attends therapeutical doses.

PATHOGENY OF INFANTILE PARALYSIS.[1]

(Paper read before the New York County Medical Society, December 22, 1873.)

There is probably no other affection than infantile paralysis which offers so remarkable a contrast between the frequency of its occurrence and general agreement in regard to the description of its symptoms, and the extreme rarity of opportunities that have been offered for its anatomical investigation. Brunniche[2] observed seven cases in one year in a general clinic; and in the same length of time I have myself observed thirteen cases of paralysis in children, of which nine were true infantile paralysis. West[3] gives a table of thirty-two cases; Hillier,[4] of twenty-four. Duchenne fils[5] tabulates observations of seventy cases. The books of Dr. Knight's hospital, of this city, contain, in the space of two years, records of one hundred cases of paralysis, of which nearly two-thirds belong to the special affection that occupies us. Volkmann,[6] who gives no table, says that he has seen over one hundred cases; and Barwell[7] makes an analogous assertion.

Nevertheless, the number of autopsies recorded since Underwood first described the disease, in 1789, is not more than twenty-

[1] Reprinted from *The American Journal of Obstetrics and Diseases of Women and Children*, 1874.

[2] *Journal für Kinderkrankheiten*, Bd. 36, 1861.

[3] *Children's Diseases*, 1860.

[4] *Diseases of Children*, 1868.

[5] *Archives Gén.*, 1864.

[6] *Sammlung klinischer Vorräge*, No. I, 1870.

[7] *Lancet*, 1872.

seven, if limited to children, or twenty-nine, if we include two cases of quite analogous disease observed in the adult. Even these few autopsies are not all known to even recent writers on the subject. In 1860, Heine,[1] in his second edition, knew of but three—those by Hutin, Longet, and Fliess. In 1864, Laborde[2] asserts that but four autopsies are known to science—the two made by Rilliet and Barthez, one by Fliess, and one by Duchenne and Bouvier. To these he added the two that formed the basis of his own monograph. In 1867, Dr. Taylor, of New York,[3] observes that nothing satisfactory has been discovered in regard to the pathological anatomy of infantile paralysis. In 1871, Gerhardt[4] quotes only four cases—those of Hutin, Longet, Behrend, and Recklinghausen. In 1870, Meigs[5] quotes these four, the two of Laborde, and one by Hammond (*Journ. of Psych. Med.*, 1851), and is unacquainted with any others. In 1868, Radcliffe quotes six cases, and affirms them to be all negative in result, including the two of Laborde.[6]

In 1872, Smith, basing his opinion upon the same cases, says that nothing satisfactory is known.[7] Finally, as late as 1873, Adams[8] asserts that only three autopsies have been recorded—the two by Rilliet and one by Fliess, to which he adds one by himself, also negative in character. Since Laborde's cases in 1864, I am aware of fourteen that have been published, and of these only two, one by Hammond and one by Adams, are known to or at least mentioned by the authors just named. In the real or supposed absence of sufficient data to form a positive theory, conjecture has run wild in framing hypotheses. In regard to them, it is useful to recognize three distinct phases of opinion, corresponding to successive anatomical discoveries.

In the first period, opened by Underwood, in 1789, the disease was defined as essential, *i.e.*, as unaccompanied by any structural lesion whatever. This is the well-known opinion of Rilliet and Barthez, and is maintained at much later dates by Kennedy,[9]

[1] *Die Kinderlähmung*, 1860. *Zweite Auflage.*

[2] *Paralysie de l'Enfance*, 1864.

[3] On Infantile Paralysis and resulting deformities.

[4] *Lehrbuch der Kinderkrankheiten*, 1871.

[5] *Diseases of Children.*

[6] Reynold's *System of Medicine.*

[7] *Diseases of Children.*

[8] *On Club Foot.*

[9] *Dublin Quarterly*, 1850.

West,[1] Bierbaum,[2] Vogel,[3] Bouchut,[4] Kétli,[5] Politzer,[6] Elischer,[7] Barwell,[8] Braun,[9] and Adams,[10] the last seven authors having written at various dates between 1871 and 1873. Barwell rather emphatically denounces the existing excessive tendency to localize infantile paralysis in the spinal cord, and reaffirms the essential, functional, peripheric nature of the disease. On the other hand, Drs. Taylor,[11] Smith, and to a certain extent Meigs imitate, to-day, the reticence of Marshall Hall,[12] in 1836, who declared himself, from lack of testimony, unable to form an opinion. Roth,[13] who gives a careful résumé of several autopsies, and even Cornil,[14] who has himself contributed one of the best known, continue this reserve.

Brown-Séquard, in 1860[15] and 1861,[16] classed the "so-called" essential paralysis of children, among reflex paralyses, dependent upon peripheric irritation, and characterized anatomically by absence of all lesion in the spinal cord. Echeverria, in 1861,[17] re-enunciated this doctrine, the latter part with much more emphasis than his master had done, and the theory was accepted with certain avidity by many English writers, as Churchill, Coley, and others, who seem to have a national preference for any theory of disease that evades the necessity of post-mortem examinations. A second modification of the essential doctrine is represented by Bouchut, who, from the essential paralyses, separates others called myogenic, on account of muscular lesions which the author considers primitive.[18]

Much before this time, however, attention had been drawn to the spinal cord as the real seat of the infantile paralysis, and

[1] *Diseases of Children*, 1848. Am. ed. of 1860.

[2] *Jahrbuch für Kinderkrank.*, 1859.

[3] *Diseases of Children.* Transl. from fourth German edition. Raphael, 1870.

[4] *Bull. de Thérap.*, 1872.

[5] *Jahrbuch für Kinderkrank.*, 1873.

[6] *Jahrbuch für Kinderkrank.*, 1866.

[7] Quoted by Kétli.

[8] Loc. cit.

[9] *Compendium für Kinderkrank.*, 1871, p. 161.

[10] Loc. cit.

[11] *Infantile Paralysis*, 1867.

[12] *Lectures on Nervous Syst.*, 1836, p. 81.

[13] *Paralysis in Infancy*, Lond., 1869.

[14] *Manuel d'Histol. Path.*, 1873, p. 637. (2e Partie.)

[15] *Central Nervous System.*

[16] *Lectures on Paraplegia.*

[17] *Am. Med. Times*, 1861, vol. ii, p. 315.

[18] *Traité des Maladies des Enfants*, 1862.

of some material lesion which should be its proximate cause. As I believe has invariably been the case in the study of diseases of the nervous system this lesion was at first located in its blood-vessels, and the paralysis attributed to a congestion of the spinal cord, or to hemorrhage, capillary or otherwise, into its substance. This opinion was advanced by Heine as a plausible conjecture, supported however by the assertions of Muller, Sandras,[1] Warnatz,[2] and Vogt,[3] and with the autopsy of Fliess.[4] It was reaffirmed by Eulenburg in 1859,[5] although in his treatise on Nervous Diseases published in 1872, he is much less positive. He assigns a central origin to the paralysis, but will venture no conclusions concerning the nature of the lesion. Brunniche[6] and Radcliffe,[7] on the other hand, do not hesitate to describe this lesion as congestion, and Adams admits a slight congestion as the only alternative to the theory of purely functional alteration.

Dr. Jacobi, in his lectures on dentition, partly combated Heine's theory as too exclusive, nevertheless inclined to admit its correctness in a large number of cases, and even assumed a spinal hemorrhage as the lesion which would correspond most completely to the symptoms, and especially to the mode of invasion of infantile paralysis. Mauthner, in 1844,[8] knew no other cause for sudden paralysis in children than cerebral or spinal apoplexy.

In the *Lancet* for 1870, Clifford Albutt emphatically rejects a "reflex" origin for infantile paralysis, and ascribes the disease, in some cases at least, to spinal hemorrhage. He relates a case, not however of infantile paralysis, but of hemorrhage into the cervical cord, of which the child immediately died. Hayem,[9] in his thesis on Intra-rachidian Hemorrhages, repeats this case, and observes that, had the hemorrhage occurred in the lumbar instead of the cervical cord, the child might have survived and offered an apparently typical case of infantile paralysis.

Finally Salomon, in 1868,[10] ascribes the paralysis to an

[1] Schmidt's *Jahrbücher*, Bd. 80.

[2] Schmidt's *Jahrbücher*, Bd. iv., suppl.

[3] *Die essentielle Lähmung der Kinder*, Bern, 1858.

[4] *Journal für Kinderkrankheiten*, Bd. xiii.

[5] *Archiv. Virch.*, 1859.

[6] Loc. cit.

[7] Loc. cit.

[8] *Die Krankheiten des Gehirns und Rüchenmarkes bei Kindern*, 1844.

[9] *Thése de Concours*, 1872.

[10] *Journ. für Kinderkrank.*, 1868.

"exsudation process" in the spinal membranes, by which the cord is more or less compressed.

In a third period, researches have been made upon the nervous elements of the cord—researches for the first time conducted by means of the microscope—and which have founded an entirely new school of doctrines concerning infantile paralysis. Yet in this school are several different sects. Laborde originally located the lesion in the anterior columns and anterior roots, and is supported in this by Cornil, who communicated a case to the Société de Biologie in 1863. Gerhardt follows the French pathologists,[1] and Meigs[2] admits sclerosis of the anterior columns and roots to be, at least, a coincidence in cases of long standing. On the other hand, Charcot,[3] Joffroy, Parrot, Prevost,[4] Vulpian,[5] Roger, and Damaschino,[6] and Lockhart Clarke[7] affirm, as the result of new autopsies published by them, that the essence of infantile paralysis consists in an inflammatory atrophy of the cells in the anterior horn of gray substance, especially on its outer side. On the authority of these same autopsies, this view of the disease is admitted as highly probable by Meyer[8] and Volkmann[9] in Germany, Hillier[10] in London, Hammond[11] in New York. In Paris, Duchenne, father[12] and son,[13] had, in 1861 and 1864, advanced nearly this theory as a most plausible hypothesis,[14] before anatomical demonstration could be obtained, and ranked infantile paralysis with the spinal paralysis of adults, and even with its acute ascending form, and also with glosso-labio-pharyngeal paralysis. But since the publication of these facts, Dujardin Beaumetz has placed infantile paralysis among cases of acute myelitis,[15] and Hallopeau has described infantile paralysis as a form of myelitis, to be associated closely with progressive muscular atrophy, as a parenchymatous inflammation of the anterior gray substance, and thus notably distinguished from the diffused inflammations that affect the neuroglia and result in sclerosis.[16]

[1] *Lehrbuch für Kinderkrankheiten*, p. 699. [2] Loc. cit.
[3] *Archives de Phys.*, 1870 *Revue Phot.*, 1872.
[4] *Comptes rendus Soc. de Biol.*, 1866. [5] *Archives de Phys.*, 1870.
[6] *Gaz. Med.*, 1871. [7] *Med. Chir. Trans.*, 1868.
[8] *Journ. für Kinderkrank.*, 1868.
[9] *On Electricity.* Translated by Hammond.
[10] Loc. cit. [11] Loc. cit. [12] *Diseases of Nervous System.*
[13] *Electris. local.*, 1861. [14] *Archives Gén.*, 1864.
[15] *De la Myelite Aigue, Thèse de Concours*, 1872. [16] *Archives Gén.*, 1871.

"If," he writes, "we have been able to localize in the posterior cornua the organ of locomotor ataxia, in the same manner we have the right to consider the anterior gray substance as the central organ of muscular atrophy. Wherever this exists alterations of the anterior horns have been found on competent microscopic examination; and these amyotrophic lesions are to be attributed to the same cause, whether they appear in the course of a diffused myelitis, or under the form of progressive muscular atrophy or of infantile paralysis."

So Charcot, in his Lessons on the Nervous System, classes together hematomyelie, acute central myelitis, and infantile paralysis, as peculiar irritative affections of the central gray substance of the spinal cord, necessarily resulting in muscular atrophy. In these affections, of which infantile paralysis is the most perfect type, everything leads to the belief that the primitive lesion is in the nerve cells, as distinguished from the neuroglia and reticulum of nerve fibres.[1] Vulpian announces the same doctrine in his *Cours de l'Ecole de Medécine*. In the *Revue Photographique* for the same year is published a lecture by Charcot upon the group of myopathies of spinal origin, a group almost exactly corresponding to that framed in 1861 by Duchenne. Finally, encouraged by this definite declaration of doctrine on the part of the illustrious master, Petitfils has sustained, in 1873, an inaugural thesis under the title, *acute atrophy of motor cells*, which is described as the primitive lesion universally existing in the diseases of this group, namely, glosso-labio-pharyngeal paralysis, progressive muscular atrophy, general spinal paralysis of the adult, and infantile paralysis.[2]

Nothing can be more complete than the opposition between this opinion and that formerly given, and which has so widely prevailed, that every writer on the subject has felt obliged to refer to the disease as either essential, or at least as the "so-called" essential paralysis of children.

Since the change of opinion—which, however, is yet very far from universal, even among competent authorities—is based on the results of autopsies, it is necessary to examine these results in detail to ascertain how far they justify such a revolution, or what objections may be made to them.

[1] *Leçons à la Salpêtrière*, 1872.

[2] *Thèse de Paris*, 1873. Considérations sur l'atrophie aigue des cellules motrices.

The appearances described are referred either to the paralyzed muscles, the spinal cord, or both, and may be grouped into three classes. In the first nothing was found; in the second, atrophy of muscles, and lesions discovered in the cord, that, however, offered no peculiarity corresponding to the peculiar symptoms of infantile paralysis; in the third, finally, lesions were found involving one or more of the peculiar elements of the cord, and analogous to those discovered in other cases of disease, which resembled infantile paralysis in loss of voluntary motion, and in atrophy of the muscles paralyzed.

1st. *Negative Autopsies.*—There are seven autopsies on record, whose results are said to be completely negative. Of these, three—Rilliet's,[1] and one by Duchenne and Bouvier, may be immediately set aside, since it is admitted that no microscopic examination was made. We think that to-day it would be superfluous to observe, as a recent English writer does with considerable naïveté, that "the researches of Mr. Lockhart Clarke have shown that the microscope *may be* of very great assistance in unravelling the pathology of the spinal cord." A fourth negative case is that reported by Mr. Adams, in his Treatise on Club-foot. He says, that after a very careful examination, he was unable to detect any morbid condition of the spinal cord, but does not specify whether the examination was microscopical, nor how long a time had elapsed since the occurrence of the paralysis. A fifth case, more important, was published by Bouchut, in the *Union Médicale* for 1867, where a microscopial examination, made by Robin, could discover nothing in the cord. Finally, in a very recent number of the *Jahrbuch für Kinderkrankheiten* for 1873, Kétli quotes two autopsies made by Elischer upon paralyzed children who had succumbed to variola. Microscopical examination of the cord gave completely negative results, but the muscles offered examples of two kinds of degeneration, the fatty and the colloid. Kétli considers these the most exhaustive researches that have been made, and as completely justifying Bouchut's description of myogenic paralysis, characterized by primitive granular fatty degeneration of muscular fibre. This view is analogous to that advocated by Friedreich in regard to progressive muscular atrophy, a disease so frequently associated with infantile paralysis by authors who assign a central

[1] *Gaz. Méd.*, 1851.

nervous origin to both.[1] The latter authors are nearly all more recent than the former.

Among the six negative cases, therefore, while four are important, only one can be considered completely satisfactory—that reported by Bouchut.

Of the next seven cases, five are old, among the first on record. They are repeated in almost every monograph or chapter on infantile paralysis. The first case is recorded by Longet in a girl with a club-foot, who died at the age of eight, the muscles, sciatic nerve, and its anterior roots on the corresponding side, were all atrophied. In the second case, from Hutin, paraplegia occurred at 7; death at 45; and at the autopsy the lower part of the cord was found atrophied. In the third and fourth cases the paralysis was evidently secondary to general organic disease of the cord; in the one case spinal meningitis (Behrend), in the other tubercle (Recklinghausen). These latter cases can only show that pressure exercised upon the cord may produce paralysis whenever the motor organs of the cord have become involved. They, of course, cannot be involved as most frequent explanation of ordinary infantile paralysis. The two cases of simple atrophy correspond to the lesions found after section of nerves.

The fifth autopsy of this class is that so often quoted from Fleiss, recorded in the *Journal für Kinderkrankheiten* for 1849. A child, 5 years old, having passed a restless night, was found in the morning with the left arm paralyzed. No adequate cause for the paralysis was discoverable, but the examination showed in the mouth some decayed milk-teeth. A few days later the child was killed by a kick from a horse, and at the autopsy was seen a notable dilatation of blood-vessels around the

[1] Friedreich gives the following table of authors in two classes, of which the first assigns a muscular, the second a nerval, origin to the disease.

Muscular

Meryon. *Med.-Chir. Trans.*, 1852–1866.
Wachsmuth. *Zeits. f. rat. Med.*, 1855.
Oppenheimer. *Ueber prog. fett. musk*, 1855.
Hasse. *Krankheiten des Nerven Syst.*, 1869.
Meyer. *Wiener Wochenschrift*, 1855.
Friedberg. *Pathol. und Therap. Mus. kellähm.*, 1858.
Roberts *Wasting Palsy*, 1858.

Nerval

Romberg. *Lehrbuch für Nervenkrank.*
Fromman. *Deutsche Klinik*, 1857.
Virchow. *Handbuch*, 1854.
Jaccoud. *Chir. Med.*, 1867.
Ollivier. *Thèse de Concours*, 1869.
Erb. *Deutsches Archiv.* 1867. Bd. v.
Trousseau. Chir., 1868.
Charcot *Arch. de Phys.*, 1869.
Clarke *Med. Trans.*, 1866–1868.
Hayem *Arch. de Phys.*, 1869.

roots of the left brachial plexus. This vascular turgescence extended to the shoulder, the neck, and submaxillary region.

The cerebral meninges were congested, as a result of the blow. No microscopic examination was made of the cord.

Fleiss attributes the congestion to the irritation of the decayed teeth, and the paralysis to the pressure of the dilated blood-vessels upon the roots of the brachial plexus. The examination was too incomplete to permit this explanation to be accepted as decisive; but this case, like those of Longet and Hutin, offers no contradictions with later autopsies.

The sixth case is reported by Hammond in the first[1] volume of the *Journal of Psychical Medicine*. Paralysis of the left leg had lasted four years, and at the autopsy was found an encysted clot, in the left anterior column of the lower part of the dorsal region. The history of the début of the disease is not given, nor are we told whether the cord showed any evidence of myelitis, or to what symptoms the patient succumbed.

It is remarkable that this is the only case of infantile paralysis in which evidences of a circumscribed hemorrhage have been found in the cord. The case related by Clifford Albutt is the following: A healthy child of seven months was lifted up rather roughly by the mother, fell forward heavily in her arms, and a few minutes later was paralyzed in its four limbs. Death occurred by paralysis of the respiration, and at the autopsy were found two hemorrhagic clots in the cervical spinal cord, the smaller in the left posterior horn, the larger in the right posterior.

In quoting this case, Hayem refers to another, the seventh in our series, where, in a person of twenty-four years of age, who had been paralyzed when two years old, he found an infiltrated hemorrhage in the lumbar cord.[2]

The third class of autopsies of presumed infantile paralysis, are all recent, and include twelve cases, in all of which some lesion was found in the spinal cord.

The first autopsy was published by Cornil in 1863. A woman of forty-nine had become paraplegic at two years of age, and could not walk for six years. After that, was enabled to

[1] *Journal of Psychical Medicine*, vol. i., p. 51.

[2] A table of these same cases has been published by Dr. E. C. Seguin, in the N. Y. *Medical Record* for last January.

walk, though painfully, by means of the muscles of the thighs, although those of the leg and foot were atrophied, especially on the left side. This false restoration of motor power I have observed many times myself. After death by cancer of the pleura, the autopsy discovered complete fatty substitution of the muscles of the left leg, and incomplete on the right; atrophy and fatty degeneration of the sciatic nerves, and diminution in the thickness of the anterior columns of the lumbar cord. A great number of amyloid corpuscles were strewn through the anterior columns. The cells of the cornua were intact.

The next two are those often quoted, published by Laborde in 1864, in which the anterior columns of the cord, translucid to the naked eye, were found by microscopical examination to be extensively sclerosed. In the mass of conjunctive elements, the nerve tubes had atrophied, many had completely disappeared, many that remained were varicose. This was especially noticeable in the first case, a child of two years, who at the age of eight months, after a short fever, was seized with general paralysis, soon limited to the lower limbs. In the second case the child had fever and repeated convulsions at a year old, then became paraplegic. Before death, a year later, atrophy and consequent deformity had made much progress. In this case death occurred from pneumonia, and at the autopsy was found a remarkable vascularization of the spinal pia mater, and of the superficial part of the anterior column. The nuclei of the capillaries were multiplied, and the walls of these vessels surrounded by exsudation corpuscles, which also were infiltrated in great numbers among the nerve tubes. The latter were varicose and broken in many places, in many others had entirely disappeared. In both autopsies the elements of the cornua were noted as perfectly healthy, as were also those of the paralyzed muscles.

The fourth autopsy is by Prevost in 1866 (Soc. Biol.). The history of the paralysis could not be obtained, but at 78, the time of death, the left leg was paralyzed, muscles soft and flaccid, the foot in talipes calcaneus. After death these muscles were found to be completely converted into fat. The inter-muscular nerve-fibres were unaltered. In the nervous centres, besides a recent purulent cerebro-spinal meningitis, not diagnosed during life, was found a marked atrophy of the anterior horn

on the left side. The external portion was converted into connective tissue, colored red by carmine, and in whose meshes hardly a nerve-cell was to be found. The nerve tubes in the columns or the anterior roots were intact.

The fifth autopsy belongs to Lockhart Clarke, and is published in the *Medico-Chirurgical Transactions* for 1868, as a case of progressive muscular atrophy. The symptoms are those of infantile paralysis; the lesions similar to those found by the author in cases of the latter disease, and consist in foci of granular disintegration in the anterior cornua of the cord, and where the nerve-cells had disappeared.

The sixth case was communicated by Charcot and Joffroy to the Soc. de Biol. in 1869. Sudden general paralysis occurred at seven years, accompanied by a transitory loss of speech. A certain weakness persisted in the four limbs, which amounted to permanent paralysis in the left arm. Death at 32. At the autopsy was found, in the entire length of the cord, a marked alteration of the anterior cornua, with integrity of the anterior columns. In the cornua the motor cells had extensively disappeared, and been replaced by conjunctive tissue. This alteration was chiefly marked in the cervical region on the left side.

The seventh case is from Vulpian, and is detailed in the *Archives de Physiologie* for 1870. Here, as in Prevost's case, was no history. At 66, age of death, the left leg was atrophied and paralyzed, and there was a coxo-femoral dislocation, which the patient affirmed existed from infancy. After death the paralyzed muscles were found to be converted into fat, and the spinal cord, scarcely altered to the naked eye, showed under the microscope a species of atrophy of the gray substance in lower lumbar cord, and a species of sclerosis of the right anterior horn. At this point the section was less colored by chromic acid, more by carmine; the majority of the nerve-cells in the external path of the horn had disappeared, and their place was occupied by new connective tissue, and enlarged blood-vessels. Besides, there was very superficial sclerosis of the anterior columns.

The eighth case appeared also in 1870, and is by Parrot and Joffroy. The autopsy was made on a child of three years, completely paralyzed in the left lower extremity, incompletely in the right. The paralyzed muscles contained an abnormal quantity of conjunctive tissue, but were not fatty. The alterations

of the anterior horns in the lumbar were precisely similar to those of Vulpian, and their relative extent on the right and left side corresponded to the degree of paralysis. There was noticed besides, atrophy of the axis cylinders constituting the nervous reticulum, to be distinguished from that of the neuroglia; atrophy and sclerosis of the anterior columns; and alteration of vessels, whose lymphatic sheaths were crowded with fat granules. The sclerosis coincided in extent with the lesions of the cornua, but the alterations of the vessels extended much further up the cord.

In 1871 appeared the memoir of Roger and Damaschino, containing the record of three new cases. In the first case, left hemiplegic paralysis at two years old, rapidly limited to the left deltoid, which became much atrophied. Death two months later of hemorrhagic scarlatina, during which an attack of paraplegia, principally at the right. The deltoidwas found in simple atrophy; the left anterior cervical roots congested and atrophied, and in the cord various foci of alterations in left anterior cervical, and also in the right lumbar region. The microscopic lesions resembled those just described; the cells were atrophied, and nerve tubes in the roots deprived of myeline; the vessels were dilated, and their walls covered with fatty granulations, and the anterior columns were sclerosed; this about equally on the two sides. The atrophy of the roots extended all along the cord. The foci of alterations were softened and visible to the naked eye.

In the second case paraplegia occurred at two years, during a discrete variola. Death six months later of broncho-pneumonia. Examination of muscles showed some degree of fatty substitution; of the cord, two foci of softening in the anterior part of the gray substance of lumbar region, one two millimetres in diameter, another larger. In these foci the tissue was almost diffluent, the microscopic lesions the same as in the other cases and these extended to three and a half centimetres above, where no alteration was visible to the naked eye. The fatty degeneration of the blood-vessels was excessive, a reticulum of conjunctive fibres occupied the centre of the focus, from which the cells had disappeared, and this was surrounded by a true cyst wall. No distinct hemorrhage complicated this circumscribed myelitis. The anterior columns were sclerosed. In the third

case, a child of three years died thirteen months after the invasion of paraplegia, with the ordinary symptoms. Foci existed in the lumbar region similar to those in the preceding case, and surrounded also by indurated conjunctive tissue. But microscopic lesions of the anterior cornua and columns extended all along the cord.

On account of these complex alterations—degeneration of blood-vessels, formation of exsudation corpuscles, atrophy of nerve cells and tubes, hyperplasia of conjunctive nuclei, secondary sclerosis of anterior columns,—the authors admit a myelitis starting, not from the motor cells, as Charcot would have it, but from the interstitial tissue of the cord.

The twelfth observation is due to Lancereaux, and is published by Petitfils in his Thesis for 1873. Paralysis of the left arm at two or three years old, resulting in considerable atrophy. Death at 18. The muscles were found in simple atrophy, the left anterior horn was atrophied in the cervical region, from disappearance of external group of motor cells, and substitution of conjunctive tissue. A certain amount of atrophy existed in the left half of the lumbar region. There was no antero-lateral sclerosis.

From comparison of these twelve observations, by far the most important on record, it results that five lesions have been found in the cord in cases of unquestioned or presumed infantile paralysis. 1st. Atrophy of the nerve cells occupying the external portion of the anterior horn, and atrophy of the nervous reticulum formed by their prolongations.[1] This in nine cases. 2d. Atrophy of the anterior roots, and sclerosis of the anterior columns, observed alone in the three first cases of this series published, and coinciding with cellular atrophy in four of the other cases, most marked in the three that offered foci of softening. 3d. Proliferation of conjunctive nuclei, occupying the place of the nerve cells; in the nine cases these were atrophied. 4th. Dilatations of the blood-vessels, and fatty degeneration of their walls, described in four cases. It is quite possible that these existed in some of the others, where they are not described, because they had not been expected. 5th. Distinct foci of softening limited to the anterior cornua on the side corresponding to the paralysis, and proportioned in extent to the

[1] See *Boll. Archiv für Psychiatrie*, 1873.

degree of paralysis. These only described in the three observations of Roger and Damaschino, where the autopsy was made two, six, and twenty-three months after the occurrence of the paralysis, and when death had been occasioned by febrile disease. In the two last the focus of softening surrounded by an indurated border, which had not had time to develop in the first case.

On the whole, therefore, the number of cases of infantile paralysis, in which lesions of the motor sections of the cord have been found, greatly preponderate over the negative cases. All recorded cases with microscopical examination, must, however, be taken into account, and their variations must be explained by variations: 1st, in the form of the disease; 2d, in the length of time intervening between the paralytic accidents and the autopsy.

Different cases of infantile paralysis vary: 1st, in their mode of invasion; 2d, in their march; 3d, in the age of the subjects.

In regard to the mode of invasion of paralysis in children, I have distinguished nine distinct forms, most of them noticed among the thirteen cases observed by myself, and twenty-four selected at random from the collection at Dr. Knight's hospital.

In the first, the paralysis is absolutely sudden, occurs in the day-time, in the midst of health, while the child is under competent observation. These cases, often represented as typical, are in reality the rarest of all—only twelve out of one hundred and sixty-three cases. I have not seen one, nor is one recorded in West's table of thirty-two cases. There is one among Dr. Knight's cases, four in Hillier's table of twenty-four, and seven among the seventy cases tabulated by Duchenne fils; giving a total of twelve in 163 cases. It is well known that the severity of the paralysis bears no relation to the mode of invasion, or these cases might be supposed to be the mildest, which is not, however, true.

In the second form, much more frequent, the paralysis is discovered in the morning, after a perfectly quiet night; eight cases out of my thirty-seven were of this class.

These recall the phenomena of spinal congestion, as described by Brown-Séquard, where the paralysis is aggravated by recumbent position, on account of the gravitation of blood to the spinal meninges, and also by the first assumption of the vertical

position, owing to the descent of cerebro-spinal fluid. The latter circumstance, however, would have no influence except in paralysis of the lower extremities.

In the third form febrile symptoms occur, generally beginning in the evening and lasting all night, or else two to three days. When the fever is slight, these cases closely resemble the morning paralysis of the second class. Eleven of Duchenne's cases were of this form. He says that the older the patient the greater is the duration and severity of the fever.

In the fourth form the paralysis is preceded by convulsions instead of fever. This in four of my thirty-seven cases.

In the fifth class the paralysis occurs in the course of another disease. In one of my cases the paralysis was observed after the child had been long kept in bed with purulent conjunctivitis; in two others occurred suddenly during an attack of cholera infantum. In one of Roger's cases, a child, already paralyzed in the left deltoid, became paraplegic during the hemorrhagic scarlatina that caused her death, and at the autopsy, nineteen days later, a focus of softening was found in the lumbar region of the cord, presenting the same microscopic lesions as the cervical focus that corresponded to the deltoid paralysis.

In a seventh class the paralysis is preceded alone by vomiting. I had two cases of this kind, in one of which the vomiting lasted two weeks and was followed by crossed hemiplegia. This case might at first be attributed to a cerebral origin, but eight years later, the muscles were atrophied without retraction, and failed to contract under faradaic electricity.

In an eighth class some mechanical accident has occurred. In none of the cases I have examined was the paralysis immediate, but preceded by accidents that were the more direct consequence of the paralysis. These are easily overlooked, without special inquiry. Thus in one of my cases, the mother asserted at first that the child had been paralyzed ever since he fell down stairs, but afterwards admitted that he was in bed a week, with high fever, before the paralysis was noticed.

Only two other such cases are on our list: in one, the child nearly fell from its nurse's arms, was caught violently by the lower extremities, and became paraplegic about a month later; in the other, fell from a wagon, and was lame in two days. In

all statistics mechanical accidents are very much in the minority, a fact in striking opposition to their frequency in the etioloy of meningeal or medullary hemorrhage.

We separate a ninth class, in which, with the usual début of infantile paralysis, symptoms are observed whose absence is generally conspicuous. This is a more important class than the others. In one of our cases the child, at the age of two and a half years, had a febrile attack, during which a physician prescribed morphine, after which she slept uninterruptedly for twenty-four hours. On awakening, she was found to be completely paralyzed and anæsthetic in both lower extremities. For two days she remained insensible to the prick of a pin, and for eight days suffered from retention of urine. This case resembles lumbar myelitis. In another case, paralysis of the left leg was preceded for two days by vague indisposition, and accompanied by fever, retention of urine, opisthotonus, and general hyperæsthesia. The absence of any modification of the sensibility, or of the action of the bladder, in the great majority of cases of infantile paralysis, renders the occasional presence of such symptoms all the more important. One similar case is recorded by West, and two by Hillier.

Although theoretically superfluous, it is often practically useful to remember, that in a tenth class of cases, the paralysis is either congenital, or has been accompanied by marked cerebral symptoms, or has existed at first under the form of hemiplegia, together with facial paralysis; and in the two last, if not in all three cases, is of cerebral origin, and therefore radically different from true infantile paralysis.

Among the thirteen cases seen by myself, twelve had been diagnosed as infantile paralysis by other physicians, and of these one was congenital and three certainly cerebral. I have based the diagnosis in the latter cases on the following points. First, on the form of the paralysis, which I have never seen hemiplegic, unless the facial nerve had been involved at the beginning.

Duchenne fils gives only one case of hemiplegia, that is not described, and two cases of cross hemiplegia, the latter admitted to be excessively rare. Heine apparently makes a class of nine cases, but in only one did the paralysis involve an upper and lower extremity. It followed a fever of several days, and

as Heine did not see the case till years afterwards, a facial paralysis might easily have been overlooked by the parents.

In two of West's cases the hemiplegia was congenital, in two it involved the face, in seven the paralysis was limited to the facial nerve; in five alone was it confined to a leg and arm of one side. In two of these it came on gradually; in one succeeded to remittent fever; in one was preceded by heaviness of the head for several days, and in one the leg was paralyzed fourteen days after the arm.

Although, therefore, the hemiplegic form cannot be said to absolutely exclude infantile paralysis, it is so exceptional as to offer a strong presumption against the existence of that disease. The second point of diagnosis is the coincidence of cerebral symptoms other than the facial paralysis, which certainly must be considered as such. It is curious how often these may be detected in quite a small range of cases. Thus: in one, the hemiplegia appeared after coma during cerebro-spinal meningitis. In a second, after a violent convulsion, the face was spasmodically drawn to the opposite side, and the patient, a child of seven, remained for a month in a state of intense maniacal excitement. In a third, developed during convalescence from scarlet fever, the hemiplegia was preceded by paresis during two days, and accompanied for a year by complete aphasia. In the fourth case, where the child, who had presented transversely at birth, offered a paralysis of the muscles of the forearm, principally, and by exception, seated in the flexors, so that the hand was bent back on the wrist, the extreme localization of the trouble was a point of much resemblance with infantile paralysis, or, as the arm had prolapsed during labor and been replaced, the paralysis might also have been attributed to a peripheric traumatism. But the first hypothesis was contradicted by the presence of an anæsthesia so complete that the child constantly chewed the ends of her fingers, and the second was equally opposed by the complete preservation of faradaic contractility. The reactions to the faradaic current are well known to constitute an important means of diagnosis between cerebral paralysis on the one hand, and those of peripheric or spinal origin on the other. The value of this test has been much disputed, but is, we believe, to-day generally admitted. Duchenne, giving greater precision to the ideas of Marshall

Hall, claims to have discovered this test. Bouchut disputes the claim to priority, but admits the value of the test. It is very remarkable, that in infantile paralysis the loss of faradaic contractility is as rapid as is loss of power to respond to electricity after section of a nerve—namely, in thirty-six hours according to Barwell, in six to eight days according to Duchenne. Salomon [1] has especially investigated this matter, and has entirely confirmed the views of Duchenne, except in regard to the absolutely bad prognosis that is implied by complete loss of contractility. It was necessary for Hammond and Radcliffe to discover, as a new fact, that the muscles which failed to react to the faradaic current, would often, though not always, respond to galvanism. In thirty-seven cases that I have examined, all of whose histories contained other indication of cerebral origin, normal faradaic contractility persisted after years of paralysis and excessive atrophy. The same is true of those singular cases of congenital paralysis accompanied by rigid muscular contractions. In all cases on the other hand, where such cerebral symptoms were absent, the muscles completely failed to contract, although their helplessness, atrophy, and flaccidity were not greater than in the first case. Since in muscles atrophied after long standing cerebral paralysis, faradaic contractility persists, and since this completely disappears in infantile paralysis long before atrophy has set in, the phenomenon is clearly independent of the condition of the muscular fibre, and must be connected with that of the nerves. It is observed in diffused chronic myelitis, as well as in infantile paralysis, and Vulpian concludes that lesions of the cord determine in nerves alterations in structure similar to those observed in their peripheric end after section.

It has seemed to me that the possibility of exciting contractions by a very slow interruption of a strong induced current, does not always imply return of power to the nerve. In one case, where, after two days' convulsions, paralysis of the right arm had occurred, soon limited to the deltoid, where it was persisting two years later, an ordinary induced current gave no contractions whatever, but these were obtained with galvanism; and also when the secondary induced current was very slow and jerking, and applied directly to the muscle instead of through

[1] *Jahrbuch für Kinderkrankheiten*, 1868.

the nerve. But after months of treatment with this current, the paralysis remained unimproved.

Another sort of fallacy is due to the derived currents, which excite contractions in antagonistic muscles, that are often mistaken for movements in those through which the current is passing, and which really are too much paralyzed to respond. Thus I have often seen the toes move as the common extensor was faradaized, but it was evident that they moved only in flexion, precisely as when the current was passed directly through the flexors themselves.

Paralysis following diphtheria or other febrile blood diseases, as described by Gubler, must also be separated from real infantile paralysis. Many cases are really due also to different accidents than the one to which they are attributed. Thus S. Weir Mitchell describes a case where a child, shortly after a fall, was found to be lame in the right leg; but it was discovered at the same time that decided atrophy of the muscles already existed, and it was shown that the nerves of the lumbar plexus were compressed by exsudations that had formed during a severe attack of typhlitis.

In regard to the march of the disease, three principal varieties are to be distinguished: in the first, the paralysis completely disappears, either spontaneously or after treatment, in from two days to a few months. Kennedy's famous cases are of this description. Barwell asserts that the majority of cases that came under his observation, are curable when treatment is begun shortly after the debut of the paralysis. A similar assertion is repeated by Hitzig and Jurgensen[1] in opposition to the extremely unfavorable prognosis of Volkmann. For the personal knowledge of one such case, I am indebted to Dr. Jacobi. A lady, affected with chronic endometritis, miscarried several times from fatty degeneration of the placenta. At the first living birth the placenta was found to be still partly fatty, and the child was subject for two years to repeated intestinal hemorrhages. These were attributed to an imperfect structure of blood-vessels, analogous to that existing in the placenta. At the age of two years the child was found paraplegic one morning upon awakening. No anæsthesia. In three to four days the paralysis was limited to the muscles of the right leg;

[1] *Archiv für Deutsche Klinik*, 1873.

in a week these still responded well to both currents. No electrical treatment was used, but ergot administered, and ice applied to the spine. Recovery was complete in two months.

In the second class of cases, the paralysis, at first generalized, becomes limited to a few muscles, and there persists indefinitely. In the third class, finally, the muscles begin very soon to waste, and the atrophy becomes so general and excessive that the limb dangles about like a loosely jointed stick, the famous "jambe de Polichinelle" of the French writers. These cases are too well known to require description or even illustration, but their frequency seems to me to have been exaggerated.

Among the twenty-seven autopsies, the muscles were examined in fifteen; were found simply atrophied in six; replaced more or less completely by adipose tissue in eight; and in one offered no appreciable alteration. There is no well-defined relation between the date of paralysis and the invasion of the muscles by fat. It is true, one of the cases above quoted of simple atrophy is Roger's where the examination was made two months after the date of the paralysis; but on the other hand, Hammond has examined the muscular fibre from the living subject in two cases in which the paralysis had lasted over four years, and found the structure unchanged. According to Charcot, the rapid wasting of muscular fibre within its sarcolemma, with persistence of the striations, is alone characteristic,—fatty substitution is always accidental.

In regard to the third variation, that is, in the age of the patient attacked by paralysis, it would seem at first that this is settled by the very designation, "infantile," "dental"; and indeed, to many it is so. All records, however, contain many cases in which the accidents occurred after two years old, hence beyond the period of the first dentition. But, as previously observed, attention has been recently drawn to certain cases of paralysis in the adult where the symptoms completely resemble those of infantile paralysis. In 1861 already, Duchenne described cases of general spinal paralysis in the adult, which he considered as quite analogous to infantile paralysis; and in his third edition he relates four cases that differ, indeed, from infantile paralysis in the presence of rachialgic pains, but resemble it in the rapid invasion, primitive generalization, and subsequent limitation of the

paralysis. In the thesis of Petitfils are recorded three cases. observed by Charcot, in adults. The paralysis was discovered in the morning, in one; after twenty-four hours hemiparesis, in a second; after four days vague indisposition, in a third. In one, paralysis was paraplegic from the beginning; in one, generalized at first, afterwards paraplegic; in one, it successfully invaded the four limbs. In one there was pain; in one anæsthesia; in one trembling. In all, faradaic contractility disappeared in the paralyzed limbs, which grew cold, and atrophied rapidly for a few weeks, then began to improve, and in one case were completely restored. Meyer relates two cases that have been quoted as examples of paralysis, but which are evidently progressive muscular atrophy. But M. Brown-Séquard has related to me a case, in an adult, which entirely resembled infantile paralysis, with extreme wasting, which was ultimately cured. Cuming[1] has seen a case of general paralysis, occurring suddenly, after exposure to cold, with nearly all the negative symptoms peculiar to infantile paralysis, but followed by darting pains in the lower limbs, some spasmodic contraction of their muscles, slight atrophy of the upper extremities, and claw hands. Return of power to walk in three months. I have seen a somewhat similar case at the Mount Sinai Hospital, but of which the termination is still uncertain. A man, having vomited constantly for two weeks without presenting any other symptoms, was seized with paralysis of the arms upon going to a pump in the court-yard. The next day the paralysis had extended to the lower extremities, and was followed by constant severe pains in the paralyzed limbs. The muscles wasted rapidly; nevertheless, in about three months the paralysis had become limited to the parts of the limbs below the elbow and knee-joints. A year later, the patient was still in this condition, the hands clawed, the feet in slight varus equinus; faradaic contractility abolished in the muscles that remained paralyzed.

Still another case is related with great detail by Bernhardt, in the last number of the *Archiv für Psychiatrie* (1873). In every essential respect it resembles the above, and is considered by the author as identical with the so-called infantile paralysis. A twelfth case is quoted from Lucas Championnière, in Hallopeau's memoir on diffused myelitis, already referred to. Eigh-

[1] *Dublin Quarterly*, 1869.

teen months before death, the patient, on recovery from confinement, was suddenly affected by general paralysis ultimately limited to the left lower extremity. She entered the hospital for an attack of typhoid fever, and it was then noticed that the muscles of this limb were extremely atrophied, and that faradaic contractility was abolished in them. The patient succumbed to the fever, and at the autopsy the muscles were found in fatty degeneration, and in the lumbar region of the cord, foci of softening in the two anterior horns. These were analogous to those observed by Roger, also after febrile diseases, in the muscles that remained paralyzed.

In the last January number of the *Archives de Physiologie*, Gombault relates a case quite analogous to these, but attended at first by severe rachialgia. Paralysis remained generalized for two years, but at three and a half years, use of the four limbs was incompletely recovered. Death occurred through some complication, and at the autopsy was found a pigmentary degeneration of the cells in the anterior horns, lesion generalized all along the cord. The anterior roots were atrophied, the anterior columns, and all other parts of the cord healthy. The paralyzed muscles were sclerosed, and the sarcolemmæ generally empty. This valuable autopsy may justly be classed with those already related of infantile paralysis.

It appears, therefore, that the age of the patient cannot be reckoned as an absolutely essential circumstance to the production of the most typical characters of the disease. All that can be affirmed is, that it is much the most frequent between the ages of six months and two years. On comparing the symptoms of infantile paralysis with the results furnished by autopsies, we find that a certain number among both, one and the other, may be invoked in favor of one or the other pathogenic theories we have enumerated. The sudden invasion, and occasionally complete spontaneous disappearance of the accidents, together with the negative results of four autopsies, have been supposed to prove, now the "essential," *i.e.*, functional character of the disease, now to indicate a transitory congestion of the spinal cord. These two theories are often grouped together, as if supposed to be very nearly identical; as when Adams says that infantile paralysis is either a functional disease, *or* else depends on some slight spinal congestion. But in reality the two ideas are completely distinct.

For the hypothesis of spinal congestion, so seriously defended by Radcliffe, presupposes at all events that the lesion, however transitory, is central. Whereas the assertion that infantile paralysis is essential, functional, immediately conveys to many, and is perhaps meant to convey, the idea that only the function of the motor nerves is abolished, and that an essential paralysis is, unless reflex, essentially peripheric. Especially in regard to infantile paralysis has the localization of the affection been considered a proof that the cause of the disease was to be sought on the periphery of the nervous system. Now the function of a nerve is unique and well understood—that of conducting impressions. So long as these impressions, motor or sensitive, continue to be generated, the function of the nerve can only be interrupted by interruption of the road along which the impressions travel; and further, the same cause that suspends the conveyance of one of impression must, in the great majority of cases, suspend that of the other, so that a complete motor paralysis, dependent on an affection of a nerve, is nearly always accompanied by anæsthesia. It is true that this is by no means always in proportion to the degree of motor palsy, and a case related by Mitchell may be paralleled by others, where sudden and complete paralysis caused by dislocation of the humerus, was accompanied with scarcely any loss of sensation.[1] Still the rule is the other way, and implies conditions directly opposed to those of infantile paralysis, where modifications of the sensibility are extremely exceptional.

But further, from the almost mechanical nature of the function of the nerve, it is difficult to imagine an interruption to this function dependent on other than mechanical or, at least, physical conditions, and it is so difficult to demonstrate an immaterial abolition of function, that indeed it has never been done. It is as easy to show that wire may become impervious to the passage of electricity, unless it be severed or clogged by non-conducting substances, as that a nerve whose structure is intact may nevertheless refuse to conduct impressions. Hysterical paralyses and anæsthesias prove nothing in regard to functional alterations of nerves, until it can be shown that the loss of motility or sensation in hysteria be really entirely independent of alterations in the activity of the cells. There

[1] *Injuries of Nerves*, p. 102

are only five cases in which paralysis of a nerve can be positively traced to causes confined to the nerve, when namely it has become inflamed, or has been severed, frozen, contused, or compressed. The experiments of Vulpian and Bastien,[1] Tillaux,[2] Waller, and Mitchell, have shown that in the last four cases the alteration of structure is as decided as in the first. "A nerve trunk," observes Mitchell, "is made up of a multitude of tubes, the contents of which are so nearly fluid as probably to be capable of more or less movement to and fro. When to such a bundle we apply a tight ligature, no matter how soon it be relaxed, we annihilate at once all power of the nerve to transmit impressions past the injured zone. After gradual and equal pressure the nerve is for a time incapacitated, but soon regains its normal abilities. It seemed to me that the reason for such loss and such return must be a purely mechanical disturbance of the tubal contents and a like mechanical restoration of their needed conditions of activity." To test this hypothesis, Mitchell submitted the sciatic nerve of a rabbit to pressure of mercury standing in a tube at varying heights. The conducting power of the nerve persisted until it had been pressed upon by twenty inches of mercury, then disappeared, but began to return in about fifteen seconds after removal of the pressure.

It is paralysis by compression that most nearly resembles the hypothetical "functional" paralysis, inasmuch as an organic lesion is imperceptible to the naked eye. Yet it is only the first stage of another, which can be demonstrated after slight contusion of nerves. When Mitchell struck a nerve smartly with a smooth broad whalebone slip, allowing a thin layer of muscle to intervene, the paralysis which ensued, although often temporary, was in degree complete. In these instances there was usually little hemorrhage, but a few fibres were torn, and a large proportion *suffered simply from mechanical disturbance, which gave them for a time a baccated look, and irregularities of outline, due to displacement of their semi-fluid contents.* If such a nerve be examined within a few days, when the paralysis has disappeared, the nerve tubes present but very slight traces of mechanical alteration, and a still later inspection rarely shows greater alteration of the nerve, save in a very

[1] *Gaz. Méd.*, 1855. [2] Quoted by Mitchell. Loc. cit., p. 92.

few fibres.[1] Finally, even section of a nerve acts otherwise than by merely separating the nerve tubes from the nerve centres, for it is well known that the structure of the tubes begins to alter in a few days after such an operation, and that the myeline segments and finally disappears before the nerve atrophies. The morbid process therefore is identical with that in the other cases, and it may be therefore positively asserted that there is no abolition of the conducting power of a nerve, without disturbance of its myeline.

The rapidity with which a nerve recovers from paralysis caused by compression or contusion far exceeds the rapidity of recovery in infantile paralysis, except in such cases as those of Kennedy's which are by no means the most common. If, therefore, a mechanical lesion exist when paralysis disappears in a few days, much more should it be present, if due to peripheric interruption of nerve function, when the paralysis has lasted for months or years. A "peripheric" paralysis is therefore just the reverse of an "essential" paralysis.

The effects of compression and contusion differ from the phenomena of infantile paralysis in that they are gradually induced, the paralysis is preceded by paresis, and by modifications of the sensibility, both absent in the disease under consideration. In infantile paralysis the loss of motility resembles that due to only one peripheric lesion, namely, section of the nerve. This is especially true in the absolutely sudden cases. The abolition of faradaic contractility and the rapidity of muscular atrophy are also striking points of resemblance. It is evident, however, that the first effect of section is not upon the nerve in itself, but only upon the relations between it and its centre, and the structural alterations of the nerve that follow are not apparent until from four to six days later.[2] A sudden arrest in the generation of motor force at the centre would be manifested in precisely the same way as a sudden interruption in the line of conveyance of such force, and indeed in no other way; just as there is but one phenomenon to indicate the cessation of chemical action in a battery where electricity is evolved, and interruption of the current from section of the wire by which

[1] Loc. cit., p. 93.

[2] Mitchell, loc. cit., p. 75. Lavuran, *Thèse de Strasbourg*, 1864 (quoted by Mitchell). Vulpian, *Arch. de Phys.*, 1869.

it is conducted, namely, absence of action. On the other hand, section of the nerve and section of the spinal cord at the point where it is given off, are followed by identical lesions of the nerve tubes, namely, loss of transparency, segmentation of myeline, irregular contour of tube wall, disappearance of tube contents, proliferation of inter-tubular connective tissue, ultimate atrophy. There is no evidence, therefore, that an alteration in the functions, *i.e.*, of the conducting power, of nerve fibres ever exists apart from some material alteration in their structure, and no suddenly produced material alteration can be even suspected in the type cases of infantile paralysis.

There remains, as the conceivable seat of the so-called "essential" paralysis, one of two alternatives—a functional alteration of the ultimate nervous fibrillæ, at the point where they enter into intimate combination with muscular fibre, or a similar alteration at the other extremity of the nerve, where the axis-cylinders, from its spinal root, form the anterior nervous reticulum of the cord, and continue with the prolongations from the motor cells.[1] The possibility of a localized paralysis of the nerve-muscle element was first suggested by the now familiar phenomena of poisoning with woorara. The peripheric action of this drug was demonstrated by its effect upon nerves isolated from their centres, and its failure to paralyze others isolated from the vascular system through which the poison was circulating. A paralysis of this nature has, therefore, always been associated with a morbid alteration of the blood. To such alteration, and the demonstrable structural lesions of muscular fibre, may be probably attributed diphtheritic paralyses, and others observed during convalescence from various fevers, so well described by Gubler; and many cases of so-called infantile paralysis, developed in such connections, are undoubtedly of this kind. But no such blood-poisoning can be suspected in the type cases of infantile paralysis, nor in its absence can any alteration of the ultimate nerve fibrillæ be supposed. There remain, therefore, the spinal motor cells as the only possible seat of functional alteration, which indeed is more conceivable of elements whose functions are so delicate and complicated. Whether infantile paralysis be essential or not, it certainly must be central in its origin. It is the first, or negative class of autopsies, four in

[1] *Boll. Archiv für Psychiatrie*, 1873.

number, which seem to support the idea that the central alterational is functional. The only alternative is between an annihilation of function in the motor cells of the cord preceding or independent of any appreciable alteration of their structure, and a similar arrest of function, as a consequence of structural lesion. All truly negative autopsies, of which there are in reality only four, speak in favor of the first hypothesis. It remains to be seen how far or in what way the results of other autopsies speak in favor of the second, or how the two classes of facts can be reconciled.

The theory of spinal congestion has been based, first, upon the same clinical facts invoked in support of the "essential" theory; second, upon others—such as the frequent appearance of the paralysis in the morning, its original generalization followed by limitation, the absence of rachialgic or of peripheric pains, the gradual improvement, or even cure; third, finally, partly upon the purely negative autopsies, partly upon the one recorded by Fleiss. It is noticeable that this latter was not in reality an example of congestion of the cord, but of the spinal meninges, and was accompanied by congestion of the cerebral meninges, justly ascribed to the accident that had caused the death. As regards the clinical history of spinal congestion, it differs from that of infantile paralysis—first, by the absence of important phenomena, characteristic of infantile paralysis, as the abolition of faradaic contractility and the rapid muscular atrophy; second, by the presence of others not seen in the latter disease, as the invariably paraplegic form of the paralysis, the various modifications of the sensibility, as tingling, aching, burning, muscular fatigue; finally, by the frequency of paresis, which never precedes infantile paralysis, whatever the duration of constitutional symptoms. There are certain cases, however, whose history does remarkably correspond to that of spinal congestion. The case I have quoted from Dr. Jacobi is a type of this kind, and is distinguished by the coincidence of conditions indicating a congenital imperfection of blood-vessels, predisposing to hemorrhage; by the preservation of faradaic contractility, and by the cure of the paralysis under the influence of agents calculated to diminish the circulation of the spinal cord.

In ordinary cases of spinal congestion, the peculiar symp-

toms depend on the generalization of congestion to the entire thickness of the cord, including its sensitive regions; and the absence in infantile paralysis is explained, in the theory, by a hypothetical limitation of congestion to the motor regions. The possibility of such limitation of vascular turgescence is presupposed no less in the theory of hemorrhage than in that of congestion. It is necessary, therefore, as the basis of an examination of these two theories, to consider: 1st, the anatomical facts relating to the distribution of blood-vessels in the spinal cord; 2d, the pathological lesions that have been really discovered in cases of spinal congestion or extravasation; 3d, the clinical history of the symptoms that have been observed in connection with such lesions.

In the distribution of blood-vessels to the cord, the following circumstances are noteworthy:

The spinal arteries are derived from the vertebral, but reinforced all during their course by anastomoses with the ascending cervical, intercostal, lumbar, and lateral sacral arteries. There are two classes of veins—those which bring the blood from the cord and belong to the real medullary circulation, and those which are interposed between the dura mater and the walls of the bony canal, and which form the so-called vertebral sinuses or intra-rachidian plexuses. These differ from the cerebral sinuses by their frequent anastomoses, are but loosely supported by the dura mater, and surrounded by a semi-fluid fat. The circulation in these extra-meningeal veins is in close dependence upon the double rhythm determined by the movements of circulation and respiration in the thorax, and liable to be affected, therefore, by lesions of the thoracic organs. These facts show a tolerably rich circulation both in the cord and its membranes; but the two last alone can be supposed to especially favor hemorrhage and that not into the cord, but in or outside of the membranes, and then not as a primitive accident, but as a consequence upon well-defined organic disease elsewhere. There are two anterior spinal arteries, and only one posterior; and the capillary network of the gray substance is richest in the anterior cornua. These are the two facts that might seem to render vascular turgescence or rupture more probable into the anterior than into the posterior segment of the cord.

Finally, the anastomoses formed between the spinal arteries and veins, and those which reinforce them, exist at the level of the spinal roots. In turgescence of the vascular system, therefore, pressure would be especially felt at this point, and might, if sufficiently intense, be supposed to interrupt nerve currents.

The force of the foregoing considerations is, however, much weakened by the following:

The arteries and veins furnished to the dura mater from the vessels contained in the vertebral canal, are separated from the cord by expansions of the vertebral ligaments. The cord is thus protected during turgescence of these vessels—at least of such as are of large size. The branches that enter the cord are of remarkably small size as compared with those of the brain, and subjected to much more numerous inflections. The pia mater into which they plunge, and by which they are sheathed, is much firmer than that of the brain. According to Retzius,[1] it consists of two layers, one lining the subarachnoid space, one closely applied to the cord. The subarachnoid space is large, and occupied by septa of connective tissue, among which circulates freely the cerebro spinal fluid, constantly tending to restore equilibrium of pressure upon the cord. The anastomoses around the nerve roots are so free and extensive that an afflux of blood towards the cord from without, that should remain limited to one or two pair of roots, is almost inconceivable. Similarly, the anterior capillary networks of the axis communicate freely at the periphery with posterior network, and with those above and below them; so that the gray substance of the cord, instead of being divided into distinct vascular territories, as is the case with the brain, contains a sort of uninterrupted vascular column, at any one point of which the blood is with difficulty obstructed. Finally, the danger of interference from action of the heart, is diminished by the nearness of the heart to the cord; and the influence of respiration is lessened from the fact, that while the meningeal veins empty into the superior vena cava during inspiration, they are free to empty into the inferior cava during expiration, so that a double provision is made against their obstruction. This is in contrast with the provision for the brain, and in accordance with the greater immediate danger to life from extensive congestion of the spinal cord.

[1] Schultze's *Archiv*, 1873.

It follows, therefore, that the normal anatomy of the cord tends to render medullary hemorrhage extremely difficult, for every provision is made against such local obstructions to the circulation as, by increasing local vascular tension, are known to be the efficient cause of hemorrhage into the brain. Nor has yet been demonstrated in the spinal arteries, the lesions, atheroma, embolism, thrombosis, which are so common in the cerebral. Liouville alone, in a single case, was believed to have discovered miliary aneurisms.[1] But none of these lesions exist in children, or would be suspected in cases of infantile paralysis.

A general venous congestion of the cord is from the anatomy conceivable, and from clinical facts demonstrable; but such localization of the congestion as would be required to explain the phenomena of infantile paralysis, is as incompatible with the free vascular communications just described, as are the symptoms of spinal congestion and those of the latter disease. It is true that four of the autopsies besides that of Fleiss, describe a dilatation of blood-vessels limited to the anterior cornua of the cord, but this was associated with alterations in the nutrition of anterior cells. Local variations in cellular activity do, indeed, determine local variations in the circulation; indeed the phenomena of capillary circulation are well known to depend mainly upon the action of cells. In such cases, it is the alteration of the cells which is the efficient cause of the disease, the congestion is consecutive, subordinate, and as an explanation of the paralysis, already necessitated by the cellular affection, may be set entirely aside.

These considerations are still further sustained by analysis of the histories of spinal hemorrhage,—reputed a primitive accident. Hayem has analyzed 100 cases of hemorrhage into the cord or its membranes, and affirms that this is the entire number hitherto recorded in science. Of the cases of meningeal hemorrhage all but five were evidently consecutive to some other lesion, as, rupture of a neighboring vessel, especially with an aneurism,[2] extension of a cerebral hemorrhage, traumatism, certain diseases of the nervous system, as tetanus, epilepsy, chorea, inflamma-

[1] Quoted by Hayem, *Thèse sur les Hémorrhagiés Intra-rachidiennes*, 1871. From this thesis much of the foregoing has been taken.

[2] Laennec, *Traité d'Auscult.*, t. iii., 4e edit. p. 443.

tions; [1] finally, to certain abdominal diseases, to fevers, alterations of the blood, or poisoning, especially with strychnine.

Of the five cases of meningeal hemorrhage that seemed the most purely primitive, in the first (Obs. Binard[2]) the vessels ruptured under the influence of a violent effort; in the second[3] and third[4] (Ollivier and Fallot) an encephalo-rachidian congestion preceded the hemorrhage; finally, in two cases, Gintrac[5] and Bigot,[6] the hemorrhage occurred amidst symptoms of long-standing, indicating a spinal pachymeningitis.

The cases of asserted hemorrhage into the spinal cord are still more ambiguous.

Two facts are common to all: 1st, the clinical symptoms of hemorrhage are preceded by a traumatism, or by symptoms of a myelitis; 2d, at the autopsy the hemorrhagic clot is found imbedded in tissue softened to a much greater extent than could be explained by its pressure, or presenting at least microscopical evidence of a central diffused myelitis.

As an illustration of the usual history of such cases, I will relate the details of one, of which I was recently enabled to witness the post-mortem examination. The patient, a man of 28 years old, after exposure in a snow-storm, was attacked by a severe pain in the lower part of the back, that, after lasting two or three days, was followed by paralysis of the left leg. This, however, gradually disappeared, so that three months later, the patient considered himself well, when one day, upon entering an omnibus, he suddenly lost all power over his lower limbs and fell to the ground. He was carried home, and although incapable of standing or walking, was able to move the legs a little when lying in bed. The paralysis extended to the sphincters, and was accompanied by complete anæsthesia of the lower extremities.

[1] Bouchut, *Gaz. des Hôp.*, 1863.
Joffroy, Soc. de Biol., 1870.
Thure, *Arch. Gen.*, 1845.
Bellingieri, *Gaz. Méd.*, 1834.
Griesinger, *Arch. der Heilkunde*, 1862.
Fuller, *Lancet*, 1862.
Calmeil, *Traité des Maladies de l'encéphale*, 1859, t. i., p. 167.
Ollivier d'Angers, t. ii., p. 350.
Bruggenmann, *Schmidt's Jahrb.*, 1836. And others.

[2] Quoted by Hayem. [3] Loc. cit. [4] *Archives Gen.*, 1830.

[5] *Path. Int.*, t. vi., p. 721. [6] *Thèse de Paris*. 1847.

In an hour or two the pain in the back returned, and became so extremely severe that, about the second day after the fall, morphine injections were used; a few hours later the pain disappeared, but the motor paralysis was so much increased that the patient could not stir in bed. In the course of three or four months motor power was sufficiently regained to allow the patient to creep about a little on crutches; but he remained generally in bed, and eschars of the sacrum and of the ischial tuberosities developed in July, about six months after the fall. He sank gradually, and died in October of pulmonary œdema, without the occurrence of any sudden accidents. At the autopsy was found, in the upper part of the lumbar cord, a hemorrhagic clot that filled a cavity about an inch long, and occupying the entire thickness of the cord. Around it for a quarter of an inch the cord was softened and altered in color.

At the earliest this hemorrhage could not have taken place before January, and then would have been preceded for three months by symptoms of myelitis. A case related by Lancereaux in the Soc. de Biologie for 1861, shows that hemorrhage may occur in the course of a myelitis without adding any new symptoms to those already existing.

In thirty cases of hematomyelie analyzed by Hayem, the symptoms were analogous to those of myelitis, and in all at the autopsy the clot was found surrounded by softening too extensive to be the mere effect of the hemorrhage. In the famous case described by Cruveilhier, although there was a circumscribed hemorrhage and a clot that extended from the level of the fourth to that of the sixth cervical vertebra, blood was also diffused throughout the entire gray substance of the cord—a lesion which almost necessarily indicates a central myelitis.[1] So in one case related by Brown-Séquard [2] small clots were found in the centre of the cord, between the origin of the second and third dorsal nerves, and the cord itself was softened and infiltrated from the third cervical to the last dorsal pair. Brown-Séquard quotes two other cases, in neither of which the hemorrhage was circumscribed. In a case by Jaccoud, the hemorrhage had occurred in the lumbar region, but coincided with an enormous cerebral hemorrhage. In a case communicated by Liouville to

[1] *Anat. Path.*, iii^{e}, Livraison.

[2] *Lectures on Central Nervous System*, p. 87.

the Soc. de Biol. (1872), two attacks of paraplegia occurred suddenly at three years' interval, and death two months after the second attack. Several distinct hemorrhagic foci were found in the lumbar cord, and the small blood-vessels in the neighborhood presented varicosities that Liouville considered analogous to the miliary aneurisms he had previously described in the arterioles of the brain. In another case, quoted by Hayem from Massot, a sudden paralysis of both arms had been followed by very rapid atrophy of their muscles, and also of those of the neck, thorax, and, to a less extent, of the lower limbs. Faradaic contractility was entirely lost. Death occurred suddenly, and at the autopsy a small hemorrhagic clot was found in the central gray substance and posterior horns of the inferior cervical cord. But a reddish color extended over the greater part of this gray substance, although the blood itself was not infiltrated. It is to the alteration indicated by this color, that must be attributed the previous paralysis and muscular atrophy, while the hemorrhage, which must have immediately preceded the death, was secondary to this.

It sometimes happens that the symptoms of an acute myelitis, uncomplicated with hemorrhage, exactly resemble the accidents usually attributed to hemorrhage itself. This is well shown by a case of Koster's, recorded in *Canstatt's Jahrbuch* for 1870. A man, hitherto healthy, found himself one morning, on awakening from sleep, to be completely paralyzed and anæsthetic in the lower extremities. No previous symptoms had occurred, except a little tingling in these same limbs during a few days. There was no pain, but soon dyspnœa, and then an eschar developed, which caused death by septicemia in two months. At the autopsy the lumbar cord was found softened and atrophied, as were also the anterior roots, but there was no trace of hemorrhage. Other similar cases might be quoted. Since, therefore, the symptoms ascribed to hemorrhage may be identical with those due to myelitis,—since in cases where hemorrhage has really occurred, it has been preceded by symptoms of myelitis,—since, finally, at the autopsy, the hemorrhagic clot is found embedded in tissues softened and altered in a way to present all the characters of myelitis,—we are justified, we think, in admitting with Hayem, Dujardin, Beaumetz, Charcot, Hallopeau, and Koster, that a primitive hematomyelie is among the

rarest of pathological accidents, and that hemorrhage hardly ever occurs into the spinal cord, unless its tissues have been previously altered by inflammation. This corroborates the inferences already drawn from the normal anatomy of the cord, that hardly any condition of hemorrhage can be found to exist in the distribution of the blood-vessels themselves. There is, therefore, the strongest presumptive evidence against the idea, that such a rare accident is the cause of so common a disease as infantile paralysis. Nor do the symptoms of such accident, when occurring, in the least degree resemble those of this disease. They are hyperæsthesia or anæsthesia, as sudden and complete as the motor paralysis,—exaggerated reflex actions, tetanic contractions, where the hemorrhage is meningeal—rachialgia and peripheric pains, paralysis of the sphincters, production of eschars, march rapidly progressive, and towards a speedily fatal termination. It is true that, as in the theory of congestion, these symptoms would depend upon the extension of the lesion to other than the anterior regions of the spinal cord; and the theory of hemorrhage in infantile paralysis supposes, as in the case of congestion, a localization of the morbid process to the anterior cornua or columns. But for the same reasons as in this first case, such localization is only conceivable as a capillary phenomenon dependent on the morbid nutrition of cells, to which, therefore, it would be quite secondary. Still less do any autopsies exist to prove its possibility. Three only have been even quoted in connection with infantile paralysis. Of these, the first, Clifford Albutt's, was followed by the death of the child within a few hours, and the hemorrhage extended rather into the posterior than anterior horns. It was never even supposed to be a case of infantile paralysis, but is related by Albutt as an example of the way in which such disease might be produced, had the hemorrhage taken place into the lumbar instead of cervical cord, where it so soon proved fatal. In the second case, Hayem's, paralysis had indeed occurred at two years, and the autopsy was made long after; but then the blood was found to have been infiltrated through the gray substance, as in cases of central, though here localized, myelitis. Finally, in Hammond's case a clot is said to have been found in the anterior column, but the examination of the cord was insufficient to decide on the coexistence of inflammatory lesions.

Among all the questions relating to infantile paralysis, the theory of spinal hemorrhage is the one that would seem to be most susceptible of elucidation by experiment. Vulpian,[1] in 1861, injected lycopodium powder into the anterior crural arteries of a dog, and, in several cases, found the vertebral and spinal arteries obliterated, and real softening with hemorrhage produced in the corresponding portion of the cord. These experiments should be repeated; they show how hemorrhage might be produced, but as they connect it with an increase of local arterial tension caused by circumstances that are not imitated pathologically, they do not really throw much light on the question which immediately occupies us. From review of the preceding considerations, therefore, we must exclude the hypothesis of congestion or hemorrhage from the pathogeny of the great majority of cases of infantile paralysis. But in the cases of which we have made a class apart, as characterized by the presence of peculiar symptoms, these very lesions may very probably exist.

These exceptional symptoms were complete though temporary anæsthesia, hyperæsthesia, retention of urine, and, in one case, opisthotonos, all indicative of more extensive affection of the central axis of the cord than can be possible in cases of purely motor paralysis. They are, in fact, the symptoms of acute but circumscribed myelitis, involving the whole axis of the cord, and possibly, therefore, complicated with minute hemorrhages. All the cases of spinal paralysis occurring in the adult, even when resembling infantile paralysis in every other particular, have differed by the presence of more or less pain; also a proof of the wider though temporary generalization of the morbid process.

The variations in the amount of constitutional disturbance, at the period of invasion, imply further variations in the extension of the morbid process, even when limited to the motor elements of the cord. The autopsy made by Prevost, as also those by Roger and Damaschino, shows that altered cells and blood-vessels may be found scattered through a great extent of the gray substance of the cord, amidst elements perfectly healthy, and far removed from the foci of paralysis. These alterations indicate an original generalization of the affection, from which

[1] *Gaz. Hebd.*, 1861.

the majority of the elements subsequently recovered, with consequent limitation of the paralysis. Constitutional disturbance was in proportion to the number of elements affected at the moment of invasion, not to those remaining permanently injured. From the fact observed by Duchenne fils, that fever was less in proportion as the child was younger, it should be inferred that, at an early age, morbid communications between the cells of nerve centres are less facile than at a later period, when they have become habituated to coördinated physiological action. Communications between cells must depend on different conditions than those which regulate communications between nerve cells and nerve fibres. The originally peripheric development of the nervous system, and the incomplete elaboration of the cellular masses of the nerve centres at birth, would explain why the former mode of transmission should be so ready, the latter so much less frequent; explain the tendency, on the one hand, to reflex irritations, and on the other, to minute localization in the spinal paralysis of children.

It has been demonstrated by Gerlach, and quite recently by Boll, that the prolongations of motor cells may be traced into direct communication with the axis cylinders of the nervous reticulum from which spring the anterior roots, while between the posterior cells and roots the communication is only intermediate. This fact may explain why, for a long time, morbid processes are communicated to nerves from the anterior more readily than from the posterior nerve cells; or, in other words, why in the child paralysis is more readily produced than pain.

We speak thus confidently of motor cells, because by exclusion we have been already left to localize in them the morbid process, functional or organic, that is the immediate cause of infantile paralysis. The considerations in regard to congestion and hemorrhage should have served to show that the morbid process was at least not dependent upon them, or consecutive to any vascular lesion. It only remains, by reference to those autopsies which have revealed some lesion of nervous elements in the cord, to ascertain, if possible, which among them may be considered primitive, and if it be the motor cells, to what known lesion or functional alteration the loss of their properties may be due.

Four different cases exist, alike in but one point—the coinci-

dence of muscular atrophy. In the first, the motor nerves alone (cases of Elischer) or of the nerves and a corresponding portion of the spinal cord also, were simply atrophied (cases of Hutin and Longet). In the second, the anterior columns and roots were sclerosed, without other lesion (cases of Laborde), or together with atrophy of the nerve (case of Cornil). In the third, the motor cells are pigmented, as in Gombault's case of adult paralysis, or atrophy, and disappear. Such atrophy, with sclerosis of the cornua without sclerosis of the columns, was present in six autopsies. Finally, in the fourth case, complex lesions are present, atrophy of the cells, dilatation of blood-vessels, fatty degeneration of their walls, fasciculated sclerosis, atrophy of nerves. Of these lesions, the atrophy of muscular fibre may be caused by any irritation of its motor nerve. When Erb crushed the nerve of a frog by a ligature, the nuclei of the muscular sarcolemmæ began to multiply in two weeks, and the fibre to waste while retaining its striations, its place being supplied by hyperplasia of connective tissue. And muscular atrophy is known to be a common consequence of traumatic lesions of nerves.

But in infantile paralysis the nerve has suffered no traumatism, yet, when examined, was usually found to have itself atrophied. Such atrophy can only result from a successive series of structural alterations, similar to those which invariably follow upon section of a nerve. It has been shown that the phenomena resulting from section of a nerve, especially the rapid abolition of faradaic contractility, can only be imitated by an abolition of the properties of the motor cells at its central end, and that when in these circumstances no condition existed capable of interrupting the conducting power of the nerve, it must be presumed that motor force had ceased to be generated. The nerve atrophy must therefore depend upon some affection of the motor cells, that must have persisted long enough to produce it; and the rapid muscular atrophy indicates that the nerve, either before wasting or during the process of wasting, had been irritated. As no cause for such irritation exists in the track of the nerve, it must be looked for in the motor cells; and hence these, either before or during the process that resulted in their abolition of function, must have been the seat of a peculiar irritation.

But irritated cells are in a condition of exaggerated nutri-

tive activity, that determines to them a local afflux of blood, and we have already seen that in the spinal cord no other cause for such minutely localized congestions could be assigned, except excited cellular activity. To this, therefore, must be attributed the dilatations and varicosities of the blood-vessels. The fat granules in their lymphatic sheaths result from metamorphosis of nutritive material, no longer needed by atrophied cells. Finally, while atrophy of nerve roots is associated with atrophy of nerves, and may be considered as an effect of this, or as a coincident lesion, due to the same cause; atrophy and fasciculated sclerosis of the columns of the cord, are invariably associated with irritative processes in the cells of the corresponding cornua, posterior sclerosis in tabes dorsalis, anterior sclerosis in myelitis, in such cases of wasting palsy as are associated with central lesion, and in many of the cases of infantile paralysis where lesions of the anterior cells were demonstrable. It is to be inferred, therefore, that it depended on similar cellular irritation even in the cases where lesions of cells were no longer demonstrable at the autopsy, as in the three where antero-lateral sclerosis was the only lesion found.

The various alterations of tissue must, therefore, each be ascribed to an irritation of the anterior or motor cells of the cord, and by this reference to a unique morbid process these varieties are easily reconciled. The differences are explained by an arrest in the morbid process at different stages of its evolution. At any stage such alterations of special elements might be produced as would permanently oppose restoration of function, even though the cells failed to degenerate. Thus, if during their period of irritation sufficient irritation had been propagated to a motor nerve to initiate morbid processes resulting in its atrophy, or in that of the muscular fibre, return of motion would be impossible, even though the cells, original source of the disorder, regained their functions. In the same way, a sclerosis that began to develop in the antero-lateral column while there were no motor impulses to be transmitted, would oppose a permanent barrier to their transmission when the generation of motor forces recommenced.

Finally, in regard to autopsies so completely negative that even the nerves and muscles were found intact, we may say that none such are recorded, for in all four cases the muscle had

atrophied, in two the alteration of nerve was also extremely marked; in the remaining two there is no mention of the nerve. Indeed, at present, the motor nerves are less frequently examined than the cord, or at least with less care, so that lesions are more often overlooked.

The lesions discovered in the motor cells, therefore, indicate the nature of the morbid process as decidedly in the cases where they are absent, as in those where they are found. Cellular atrophy is a proof that the molecular nutrition of the cells has been arrested. It is evident, however, that the abolition of function, so nearly sudden, must coincide with the first disturbance of nutrition, and not only with its ultimate consequence, cell atrophy, which must be accomplished gradually. While it is as conceivable that the chemical metamorphoses in the cell may be instantly arrested by means of an impression conveyed to it by a nerve, as that the chemical processes going on in a solution of inorganic salts should be arrested by the passage of a current of electricity. Both cases illustrate the now familiar law of the correlation of forces, of the relations between chemical affinities and electrical or neural actions.

The alterations of motor cells in infantile paralysis serve, therefore, as a point of transition between so-called functional disorders and so-called organic diseases, and show with exquisite precision the manner in which alterations of tissue may be determined by perversions in the nutrition of cells.

Cases other than those of infantile paralysis are not altogether rare, where the annihilation of function in important nerve cells has been so complete, that death has occurred in a few days, and before atrophic lesions had had time to develop. Tetanus has long been a familiar example, and here, as in infantile paralysis, more accurate microscopical researches are beginning to discover lesions of the cord, when life has been sufficiently prolonged. Certain curious cases of acute ascending paralysis fall under the same category. In the one related by Pellegrino Lewins in the *Archives Générales* for 1865, the death is probably due to annihilation of the functions of the brain. But another quoted in the thesis of Petit fils, where the autopsy was made by Cornil and Ranvier, is more conclusive. In the midst of apparent health occurred a sudden paraplegia, accompanied by fall of temperature and analgesia in the affect-

ed limbs, pain in the lumbar region of the back, abolition of reflex movements. Anæsthesia without paralysis extended to the upper extremities, and death supervened on the fifth day in cyanosis, from failure of the motor forces of respiration. The most careful examination of the brain and spinal cord could discover no lesion, even microscopic.

In regard to the manner in which the nutrition of the anterior cells may be arrested, it is well known that two theories are in presence. According to one, a peripheric irritation causes a spasmodic "reflex" contraction of the blood-vessels of the spinal cord. According to the other, this irritation is directly propagated, by means of an afferent nerve, to a cell whose nutritive metamorphoses are arrested, as might be the chemical reactions in a retort by the passage of an electric current. The clearest expression of this theory has perhaps been given by Mitchell, in the paper contributed to this polemic by him,[1] and reindorsed in his recent book, *On Injuries to Nerves*. "It appears to him possible that an injury may be competent so to exhaust the irritability of the nerve centres, as to occasion more or less permanent loss of function. A strong electric current is certainly able to cause such a result in a nerve trunk; and reflecting on the close correlation of the electrical and neural force, it does not seem improbable that a violent excitement of a nerve trunk, however brought about, should be able to completely exhaust the power of its connected nerve centre. . . . There is no reason why, if shock be competent to destroy vitality in vaso-motor nerves or centres it should be incompetent to so affect the centres of motion or sensation." Handfield Jones[2] declares as the result of many clinical observations, "that any afferent nerve may act as an inhibitory nerve upon the centre or centres with which it is connected, disordering or paralyzing its action." In the first number of his Archives, Brown-Séquard has detailed many illustrations of such inhibitory actions, affected by the most diverse sensitive nerves on the most different central ganglia. Eulenburg quotes the experiment of Lewisson,[3] who by strong irritation of the cutaneous nerves of a frog, suspended motor power, not only in the irritated limb, but in the

[1] *New York Medical Journal*, 1866. See also Jaccoud, *Paraplégie et l'Ataxie.*

[2] Functional Nervous Disorders, pp. 9 and 16, 1870.

[3] Lehrbuch, p. 428, quotes *Archiv. Reichert* and *Du Bois-Reymond*, 1869.

others, and considers it a proof that the centripetal irritation of sensitive nerve is sufficient to arrest the functions of the nerve centres. The anatomical facts of infantile paralysis show finally that the function of such centres is arrested by interference with the chemical processes in the nutrition of the nerve cells.

The immense pathological importance of the study of infantile paralysis may be best appreciated by enumerating its different pathological relations, which the foregoing pages have tried to set in relief.

1st. It links together tne most conspicuous external deformities, involving entire limbs, with lesions of internal microscopic groups of cells, so minute as, until recently, to have escaped observation.

2d. By exquisite localization of pathological lesions it confirms the doctrine of localization of function and independence of morbid processes in special groups of nerve cells.

3d. It helps to establish a group of diseases bearing various relations of cause or effect to this same group of cells—the anterior spinal—as adult spinal paralysis, progressive muscular atrophy; finally, even bulbar paralysis, where the disease is confined to the groups of motor cells in the medulla.

4th. With these others it helps to show the immense and peculiar influence exercised upon the nutrition of muscles by the nerve cells influencing their motor nerves. This influence is in both resemblance and contrast with that exercised on the nutrition of the skin and subcutaneous tissues by the groups of cells connected with the posterior roots and sensitive nerves. Lesions of these produce eschars, as of those, atrophy, sclerosis, or fatty degeneration.

5th. As a localized myelitis, certain cases, at least, of infantile paralysis are to be considered in their relations to other forms of myelitis, localized or diffused, parenchymatous or interstitial. They are to be contrasted with cases of tabes dorsalis, in which the myelitis localized in the posterior cornua determines a fasciculated sclerosis of the posterior columns, relatively more frequent and important than the anterior sclerosis, contrasted also with the anterior lesion of wasting palsy, which, from the slow march of the disease, may often depend on an extension of irritation from the periphery; contrasted with acute

diffused central myelitis, with equally rapid march, but where the lesion involves both neuroglia and nervous elements.

6th. As originally confined to the latter, the lesions of infantile paralysis offer one of the best illustrations of the "parenchymatous inflammation," long ago described by Virchow.

7th. By its sudden invasion infantile paralysis is symptomatically allied to such accidents of the vascular system as congestion or hemorrhage. But as these are shown to be either absent or rare, or consecutive to an affection of nerve cells, the capacity for independent morbid action possessed by these latter receives another confirmation.

8th. These affections serve as a link between the so-called reflex or inhibitory paralysis and those dependent on marked lesions of the cord.

9th. Finally, they trace minutely the successive steps in a morbid process that, beginning in a functional alteration of cellular nutrition, terminates in organic destruction of tissue, and thus dissect apart the complex phenomena both of inflammation and of general cell life.

APPENDIX

To the cases described in the preceding pages, I am enabled to add another, observed since the reading of the paper.

On the 18th of February a paralyzed child died at Dr. Knight's hospital, whose history was as follows. When a year old, the boy had had an attack of dysentery, and on recovery was found to be paralyzed in all the four limbs, and even in the muscles of the neck and back. These regained their power first, so that after a few weeks, the child was able to sit; then recovered the use of his arms, but the paralysis persisted in the lower extremities, being most marked on the left side below the knee. Admission to the hospital eight years later with paraplegia and atrophy of the paralyzed limbs. There was then not the slightest reaction to galvanic or faradaic electricity on the left side, but some response to the induced current was obtained on the right. The general health of the patient was excellent, and remained so to the day of his death. On the morning of that day he arose at 5½, still apparently well; at 6½ vomited, and was found sitting down in a corner of the ward, complaining of feeling ill. While the attendant was questioning

him, he suddenly turned pale, *fell forward* on the floor, became almost instantly pulseless, and in five minutes was dead.

The autopsy was made by Dr. Janeway in the presence of Drs. Knight, Gibney, Milner and myself. The paralyzed limbs, spinal cord, and brain were all examined with care. The *muscles* of the left leg were almost entirely converted into fat. The right gastrocnemius was equally fatty, but the deep muscular layer was tolerably preserved. To this fact was due the degree of electrical reaction that had been observed during life, as also a certain amount of voluntary control of the limb.

The cervical region of the *cord* was somewhat injected, and a little blood was infiltrated between the dura mater and the arachnoid. This came from the cranium. In this same region, careful inspection showed that the antero-lateral column was somewhat diminished in size on the right side. In the lumbar region, on the contrary, the atrophy existed on the *left* side, and by the aid of a magnifying glass was seen to extend to the left horn of gray matter.

It has not yet been possible to make the microscopical examination, but its results will be published as soon as obtained.

The cause of death was found in the *brain*. A hemorrhage had taken place into the left posterior lobe of the cerebellum. About an ounce of blood was contained in a cavity the size of a walnut. Blood had fused along the base of the brain to the anterior fossæ, and also, as before observed, had descended into the spinal membranes. The entire brain, and especially the left half of the cerebellum, was much injected.

The first symptoms presented by the child evidently coincided with the commencement of the hemorrhage, and when the effused blood became sufficient in quantity to press upon the medulla (with which, at the autopsy, the outer edge of the clot was found almost in contact), death occurred, with the *choc en avant*, so characteristic of sudden lesions of the medulla or cervical cord. Examination (by Dr. Janeway) of the blood-vessels of the cerebellum, found them extremely fatty.

Fatty degeneration of the encephalic blood-vessels, and hemorrhage into the cerebellum, are lesions so rare in a child of nine years old, as already to render this autopsy of especial interest. But more important for our present purpose, is the examination of the cord in a case of paralysis dating from infancy, and that,

even before the mcroscopical examination, can already be said to show the lesions now to be considered as characteristic, namely, atrophy of the antero-lateral columns, and of the anterior cornua. Nevertheless, we doubt that this case can be claimed as a type of Infantile Paralysis. A general paralysis after a febrile disease, as dysentery, may, with at least as much probability, be attributed to primitive degenerations of the muscles, to which the atrophy of the motor elements of the cord was only secondary.

REMARKS UPON THE ACTION OF NITRATE OF SILVER ON EPITHELIAL AND GLAND CELLS.[1]

READ AT THE MEETING OF THE NEW YORK STATE MEDICAL SOCIETY, 1874.

Mr. President: When I learned that I was to have the honor of being present at this meeting of the Association, I hoped to be able to submit to it and to you the results of some extensive experiments upon the topical action of medicines. These experiments I have indeed begun, but have been unavoidably hindered in carrying them far enough to arrive at many satisfactory results. Instead, therefore, of a memoir worthy of your attention, I am able only to offer a brief note upon a few details that, however, I trust are not devoid of interest.

The importance of topical medicine will always vary, in public esteem, according to the stress that is laid upon local diseases. There have been many periods, in the history of medicine, when the attention of physicians was so much absorbed by the general forces of the economy, that local diseases, or local manifestations of constitutional disease, were neglected. It was assumed, or rather it has been assumed more than once, that if the unknown vital forces were restrained, or sustained, or encouraged, or depressed, or stimulated, or purified, that visible lesions would disappear of themselves. Even when local treatment was used, it was often only for the purpose of attacking the general principle at a presumably vulnerable point. Hence the eulogiums passed upon the value of aromatics and balsams in the treatment of wounds. These famous remedies were designed not to heal the wound directly, but to revive the vital spirits

[2] Reprinted from the *Transactions of the New York State Medical Society,* 1874.

fainting because of it. To-day, aromatics are replaced, by disinfectants, or only retained in virtue of a disinfectant property which they may perchance possess. The wound is regarded, not merely as the gaping door through which the soul may be breathed forth towards Hades, but as an active focus of infection, from which may flow inwards a constantly rising stream of poison. The efforts of modern medicine are directed, wherever possible, less to the sustenance of vital force than to the destruction of the agents by which such force may be destroyed. In the history of an immense number of diseases, therefore, the attention of the physician is to-day, and most profitably, directed to one of two points: 1st, the existence of a focus of infection; 2nd, the existence of a drain. To the destruction of the one, or the closure of the other he bends his most powerful energies. I need scarcely recall the specific cases that illustrate most strikingly this present attitude: that surgical and puerperal fever are regarded as the results of local auto-infection; that the dangers of osteomyelitis are known to be those of pyæmia; that to the reabsorption of pus is attributed the principal danger in small-pox; that even tuberculosis has been traced to a local origin in cheesy deposits, sometimes unique. On the other hand, the importance of chronic inflammations and suppurations as permanent drains upon the system, can only be adequately appreciated when as has been done, the exuded material has been analyzed, and its composition compared with that of the blood and tissues whose nutrition it exhausts It is in this way that a chronic bronchial, intestinal, or uterine catarrh may be justly compared in its effects to a chronic albuminuria.

Catarrhal inflammations are among the most frequent of all diseases, and interest the localist because of the definite changes in anatomical tissues they offer to his observation; and interest the constitutionalist by reacting upon the general system in three ways. They are a door of drainage, a point of irritation, and, when acute, are frequently also a focus of auto-infection. To-day nearly all of this class of diseases are treated by topical medication, and this is intended, or desired, even when its application has not yet been rendered possible.

It is evident that every substance directly applied to a tissue, in order to modify the nutrition of that tissue, can only be handled to complete advantage when its precise action upon each of the

elements has been demonstrated. A complete demonstration—chemical, physiological, and morphological—is very far from being at present in our grasp for the majority of tissues or medicines. But the morphological changes, or the alterations of form, in the elements, submitted to the influence of the drug, are, in many cases, easy to observe under the microscope.

Of all elements whose reaction to topical medicines is interesting to us, that of the various epitheliums is most intensely so. "All the surfaces of the body which are in contact with the external medium, as the skin, respiratory passages, digestive tube; all which inclose blood or lymph; all the walls of closed cavities, serous, glandular, sensorial, are, with rare exceptions, lined by the cells variously known as epidermic, endothelial, epithelial—the latter spherical—cylindrical, pavement, as the case may be."[1] Everything that passes into the blood, and everything that passes out, must traverse one or more layers of epithelium. Upon its integrity, therefore, depends the nutrition of the entire body. No mucous and no serous membrane can become inflamed without involving at the outset this almost ubiquitous tissue. A large number of skin diseases depend in the morbid changes to which it is liable. It prevents or facilitates the absorption of many miasms or specific poisons, and its morbid reactions furnish the most delicate tests for their elimination. The characteristic products of many diseases are principally masses of degenerate epithelium; thus the exudation that blocks the alveoli in many forms of pneumonia, the casts of nephritis, the dejections of cholera. Nor is the interest of epithelium diminished when we examine its history, and the part it plays in the normal or pathological genesis of tissues, since Waldeyer has traced the ovule to an epithelial cell, and Thiersch believes to have demonstrated the origin of cancer in epithelial tissue.

Among topical medicines most frequently used, there is one that, in researches remote from therapeutics, has been shown to exercise a special action upon epithelial cells. I allude to nitrate of silver. In therapeutics this drug is used to meet three classes of indications: to modify inflamed mucous membranes; to destroy morbid tissue; finally, after absorption, to arrest functional or organic diseases of the nervous system. This last indication is also among the most ancient discovered; for, as Charcot and Ball

[1] Farabeuf. *De l'Epiderme et des Epitheliums.* 1872.

remark, "the use of nitrate of silver was only generalized after the promulgation of the theory of microcosm and macrocosm, in which every terrestrial metal was made to correspond, on the one hand, to a celestial body, on the other, to some organ of the human frame. It was on account of the mysterious trinity which united silver to the moon, and the moon to diseases of the brain, that salts of silver were used in the treatment of nervous affections."[1]

The practice continues in modern times, but is to be justified, if at all, upon far other grounds. Ranvier has treated nerves with a solution of nitrate of silver, containing 1 part of the salt to 300 of distilled water. This was poured upon the nerve before removing the latter from the body. In a few minutes the nerve, which was translucid and elastic, became opaque and rigid. It was washed and examined in glycerine. At a low magnifying power could be distinguished, 1st, the sheath of connective tissue, lined with large pavement epithelium; 2d, a number of little black crosses, studding the surface of the nerve. With a magnifying power of 600, the vertical branch of each cross was found to consist of the cylinder axis of a nerve fibre, blackened to the extent of the figure, by a deposit of silver; and the transverse branch was formed by a ring of silver deposit, surrounding the nerve fibre, and partly strangulating it. Although not yet demonstrated, the dilution of the agent used to determine this effect, renders the topical application in some degree comparable with internal administration, when the silver brought to the nerve tissue shall have been diluted by the whole mass of the blood.

The action of nitrate of silver upon mucous membranes is generally and curtly described as "astringent," and due to its property of coagulating albumen. The caustic action, admitted to be very superficial, is interpreted as an exaggeration of this astringency. Not only the albumen of surface secretions, but the albuminous constituents of tissues, it is said to be coagulated; their vitality is arrested and an eschar formed. As a catheretic, nitrate of silver is particularly praised on account of the precise limitation of its action, and because the dense coagulum it forms seems to constitute an effectual barrier to its reabsorption. Now we shall have occasion to show that a strong non-caustic solution

[1] *Diction. Encycl.*, vol. vi., p. 63.

of nitrate of silver may form precisely such a barrier, while on the other hand, the phenomena of reaction that occur beneath and around the eschar really extend its influence to a considerable distance. Few microscopic researches have been made on this subject as yet, but two series of experiments have fallen under my notice, whose bearing on this point is important. The first experiments to which I allude are by Alexander Stuart, and are described in the first volume of Schultze's Archives. Stuart cauterized the thigh muscles of a living frog with nitrate of silver to the extent of half a square centimetre in width, and one centimetre in depth. Two classes of alterations were seen, those representing the pure chemical action of the caustic, and others, the result of the secondary inflammation. At the point of cauterization the muscles assumed a diaphanous white color, and the muscular fibres degenerated to a granular or finely fibrillar mass, distending the sarcolemma. This mass presented all the chemical characters of coagulated albumen.

The non-cauterized muscular fibres exhibited various changes, progressing towards the final conversion of their protein substance into fat. The transverse stratum appeared white and faint, the color was transparent and whitish, sometimes opalescent. Chemical alterations (which I will not stop to detail) preceded definite changes in form. Later, granules appeared, destined to become fat; the sarcolemma thickened, the nuclei increased in size, and ultimately proliferated. These changes were accomplished in from two or three weeks. The fibres became completely fatty in from two to three months.

The other experiments to which I have referred are those, so famous, of Cohnheim, who applied nitrate of silver to the tongue of the living frog. His observation was directed principally to the changes in the blood vessels. His observations are so widely known that I will only briefly recall them. In the zone immediately surrounding the eschar, the arteries leading to it rapidly dilated; then the veins and capillaries. The circulation at the same time is accelerated. Then the vessels feel the effect oi the obstacle offered to the circulation by the eschar, and in those leading directly to it; the circulation slackens, and finally stagnates as far as the nearest collaterals, and in these the acceleration of circulation continues. These alterations are purely mechanical. In an hour or two after the application of the cautery the dilated

arteries most remote from the eschar begin to contract, and the calibre as well as rate of circulation is restored everywhere except at the point of cauterization, and two zones surrounding it. In the inner of these two is complete stagnation; in the other, persistent dilatation of all blood vessels; after six and eight hours begins the diapedesis of white and red blood corpuscles.

Cohnheim observes that other caustics, as potassa, or nitrate of mercury, act in the same way, while mechanical irritation, unless long continued, produces a far inferior effect. The chemical action of the caustic was more powerful in affecting the coasts of blood vessels. These experiments are quoted merely to show with what limitations we may accept the dictum that the effects of lunar caustic are precisely confined to the point at which it is applied. Therapeutically, such microscopic observations are of value, when we are called upon to judge such propositions as that of leaving a piece of solid nitrate of silver to melt in the cavity of the uterus. It is certain that the action immediately *produced* by lunar caustic only represents a portion of its action; the secondary effects on surrounding tissue are more important, sometimes more beneficial, often more dangerous. Among all the mucous membranes that have been treated in all variety of ways with nitrate of silver, that of the neck and body of the uterus is certainly the most often in cause. The cauterization of the pharynx and larynx with concentrated solutions, or with saturated spray, are equally familiar applications; the alleged introduction of nitrate of silver into the bronchial tubes by means of the probang, is one of the most piquant details in the history of the nitrate, perhaps in the history of contemporary medicine. But it is not my object to enumerate the various indications for utilizing the action of nitrate of silver upon mucous membranes. I wish merely to compare certain details of this treatment with details in the microscopic preparation of the same tissues. It is well known that when fresh membranes, covered with epithelial or endothelial cells, are bathed for a few minutes in solutions of nitrate of silver, that these cells are distinctly outlined by intense black borders corresponding to their natural boundaries. To obtain this effect it is necessary to use extremely dilute solutions. Klein recommends one part to two hundred or four hundred of water. Robinski uses solutions of one part to five hundred, to eight hundred, or even to one

thousand; and the latter preparations are preferred by Alferow. This fact is of importance for our purpose, for it renders the result of the direct application of silver salts to epithelium in some respects comparable to those that we might look for when it had been absorbed from the stomach, and brought to the tissues diluted by the whole mass of the blood. No single therapeutical dose, it is true, would give even this proportion;[1] but it is the property of many mineral substances to accumulate in tissues, and this is well known to be pre-eminently true of silver. This remark by the way. By whatever mechanism the black silver lines may be produced, the service that their discovery has already rendered to histology is immense. The paths of lymphatics have been traced more delicately than even by Sappey's injections; and if we may believe Recklinghausen, Auerbach and Klein, the ancient views on the structure of lymphatics have been revolutionized, since all are shown to possess an epithelial lining. Stomata have been discovered on serous membranes, analogous to those on the epidermis of plants, and leading like them to sub-epithelial spaces. New dangers—or rather more adequate explanation of old dangers—have been found in inflammations of these membranes, now viewed as immense lymph sacks; and the tendinous centre of the diaphragm has been invested with a special function of absorption hitherto unsuspected. This silver method has been used by Cohnheim to demonstrate the passage of red corpuscles between the endothelium of blood vessels, and by Alferow to test the passage of white corpuscles between the endothelium of the mesentery. We have just seen what application has been made of it by Ranvier. It would seem, therefore, that the new role of nitrate of silver in histology bids fair to rival its ancient prestige in therapeutics.

We have mentioned the fact, so well known, that when very dilute solutions of nitrate are used, and are only in contact with the membrane for a few minutes, and this only exposed to the light for a few seconds, that the black deposit occurs only on the boundary lines between the cells. But with every increase in the strength of the solution, in the prolongation of its contact, or of the subsequent influence of the light, the coloration extends

[1] With 18 lbs., or 147,040 grains ($18 \times 16 \times 480 = 147{,}040$), Therap. internal dose = $\frac{1}{4}$ to $\frac{1}{2}$ gr.

toward the centre of the cell, invading the nucleus, if at all, the very last. Sometimes, as in the mesentery of the frog, the nucleus remains clear in the midst of a uniformly brown cell. His has said that on the cornea a weak solution of nitrate of silver produced a deposit *within* the cornean corpuscles; while with a strong solution these remained pale and colorless and the deposit was formed in the intercellular substance. Schweiggel Seidel attributes the lines to a precipitation of albuminous fluid lying in furrows between the convex surface of the cells. Klein and, I believe, Recklinghausen, explain them by precipitations in an intercellular albuminous substance that holds together the individual cells.

Robinski denies the existence of this substance, and considers the lines to be an optical effect due to the position of the cells. These are uniformly colored from the beginning, but according to Robinski, this coloration must appear more intense first upon the edges because these are seen.

For our purpose it is essential to estimate the effects of the deposit, within or without the cells, upon their vitality and detriscence. It cannot be determined, *à priori*, whether the partial coagulation of an albuminous intercellular substance should retain the cells in place, or facilitate their fall; it is certain, however, that the effects of the silver must be very different upon living membranes and those removed from the body, and also that it must vary extremely with the strength of the solution. The very weakest solution used therapeutically should, from what precedes, produce a deposit that should extend through and color the entire cell. Whatever effect on the vitality of the cell would be occasioned by a deposit on its edges would be complicated, therefore, by that formed in its interior.

These considerations should suggest the necessity for an immense number of experiments. Of the series that I have sketched out for myself, I have so far only accomplished the following:

1st. Treatment of fresh mucous membrane from stomach of recently killed rabbi', with one-half per cent. solution of nitrate of silver.

2d. Administration to rabbit of one grain nitrate in a fluid drachm of distilled water.

3d. Treatment of human uvula, immediately after excision,

with a forty-grain solution of nitrate of silver, that frequently employed in pharyngeal catarrh.

4th. Application to mucous membrane of pharynx of eight grains solution of nitrate, and immediate excision of small pieces of mucous membrane so bathed. Before experimenting upon the complicated mucous membrane of the stomach I made a certain number of preparations of the normal stomach of the dog and rabbit. Some of these preparations I have brought with me. They are made according to Heidenhain's method, as described in his original memoir and exhibit the principal details which have since been accepted in the most recent text-books on the rabbit. A vertical section of the mucous membrane of the stomach was said by Heidenhain to exhibit, at a low magnifying power, four distinct parts. In my preparations, three of these are distinct. Proceeding from the internal surface, is seen, 1st, a border, deeply colored by carmine, fringed, or irregularly sinuous on the free edge, with outlines of cells faintly discernible; 2d, a narrower space, almost colorless, and where the outline of cells can scarcely be discerned; 3d, the gland tubes, lying parallel to one another, separated by a little connective tissue, filled with much larger cells, colored principally on the two walls, and leaving the centre pale. The gland cells are much less colored by carmine than is the epithelium.

In the dog, the sinuosities on the free edge are really much deeper, but the proportion of the red border to the rest of the tubes is much less than in the rabbit.

At a higher magnifying power (200 diameters) additional details are perceived. First, the red border is seen to consist of the cylindrical epithelium lining the depressions in the mucous membrane, into which one or more gland tubes open. This epithelium gradually passes into round cells, and these into larger, more polygonal ones; the clear space below the epithelial border is seen to consist of the latter, intermediate between the round and the peptic cells. Finally, these latter, as shown by Heidenhain, and afterwards by Rollet, are of two kinds—border cells, colored by carmine, and central or principal cells, scarcely colored. Heidenhain shows that the latter are tumefied during digestion, while the former remain unchanged. This fact would imply that they were the seat of the peptic secretion, while the border cells, colored like epithelium with carmine, resemble it also

in function, being analogous to the epithelial cells that exclusively occupy the non-peptic glands of the pylorus.

In the dog I have remarked one detail, not mentioned by Heidenhain or Rollet. The epithelial border seemed to consist of two layers of cells, of which the external was deeply spiculated. The lower part of the cell, deeply colored, was thus surrounded by quite a broad, clear border. This appearance suggested an analogy with the spicules described by Frey on' the pavement epithelium of the mouth and pharynx, and that as in this locality the epithelium was held more firmly in place, the clear space below the epithelium is much less distinct in the dog; the gland tubes larger, and the polygonal cells larger and more distinct. The arrangement of the border cells is more regular.

These points ascertained, I sacrificed a rabbit, and immediately removing the mucous membrane of the stomach, washed it, and left it for a few minutes in a half per cent. solution of nitrate of silver. It was then exposed to the light for ten minutes, but became intensely colored, the coloration increasing during the subsequent hardening in alcohol.

On microscopic examination of sections, uncolored by carmine, a dark brown border was observed occupying precisely the place of the carmine coloration; that is, the whole layer of epithelium. In a few places could be seen that the brown deposit was much darker *around* the epithelial cells than within them, and filled up the whole of the open mouth of the gland. In some places a brown coagulum lay in the free surface of the membrane, over the epithelium, and apparently formed of coagulated mucus. But this was by no means constant. Any albuminate that had been so formed had evidently been removed by the washing. The facility with which this was done implies that the pressure of such coagulum upon the subjacent cells, could not, as is often asserted, exercise much influence. In the colored specimens the carmine covered the silvered epithelium, but was very much darker in tint than usual. It was evident that the presence of silver in the cells, demonstrated in the uncolored preparations, did not deprive these elements of the power of taking up carmine. A brown border was formed around the cells, and in the centre of the funnel-mouths of the tubes, as before. The peptic cells were quite unaltered. Nevertheless, scattered over the surface of the tubes, appeared a number of very fine

black granules. These were the only sign that this dilute solution of nitrate of silver, applicable to membranes deprived of their vital connections, although themselves still alive, could penetrate below the epithelium covering.

A similar fact was shown by examination of the mucous membrane of the human uvula dipped in a solution of nitrate, containing forty grains to the fluid ounce, or rather more than 8 per cent. The surface became very brown, but not nearly as dark as the rabbit's stomach, and while hardening in alcohol, a large amount of brownish precipitate separated. The difference in color may have been due to the difference in epithelium, the dense, stratified epithelium of the uvula being more resistant than the cylindrical cells of the stomach. But besides the contact was much less prolonged. Sections showed a clear brown border, in which the outlines of the external layer of epithelial cells were distinctly marked. This border did not, however, extend throughout the epithelium, four or five layers of which remained perfectly colorless. The epithelial conjunctive tissue and glands were entirely unchanged.

The mucous membrane excised, after painting with an eight-grain solution (about $1\frac{3}{4}$), gave absolutely the same results, except that the brown border was narrower. It also, however, was formed by the outer layer of epithelium, and not by a layer of coagulated mucus.

All the above preparations I have brought with me.

The administration of one grain of nitrate of silver to a living rabbit was, however, followed by quite different results. After the first dose the rabbit did not appear to suffer, and the same was repeated eighteen hours later. Three hours afterwards the rabbit was found dead. This was unexpected; for although rabbits, as being unable to vomit, are more susceptible to the irritation of nitrate of silver than dogs, yet they are known to tolerate much larger doses than this, at least if reached gradually. Bogoslowsky gave a rabbit from one to ten grains daily for thirty-six days.[1] At the autopsy of the rabbit the stomach offered many symptoms of acute catarrh. It was not hyperæmiated, but slightly grayish in spots, covered with abundant flakes that looked like coagulated mucus, and eroded to different depths in various places. The stomach was washed in distilled water,

[1] *Archives Virchow*, Bd. 46, 1869.

the flakes placed in glycerine, the thinned mucous membrane hardened in alcohol, and each examined.

The flakes so exactly resembled those mentioned in all descriptions of acute gastric catarrh, and summarily dismissed as coagulated mucus, that I had not the least expectation of finding anything else. Great was my astonishment to find them exclusively composed of large polygonal cells, arranged in columns two or three thick, and in many places exactly simulating the peptic glands, of which they were evidently the casts. The cells were evidently much larger than those I had previously seen on a rabbit, larger even than the peptic cells of the dog. The nucleus was also large and remarkably distinct. The cells were partly filled with granulated matter. So far their condition resembled that described by Heidenhain as characterizing the period of digestion. But the source of their irritation was seen in the grayish color that many had assumed, as if from a very minute deposit of silver. Many cells were perfectly clear and pale.

To a specimen of these flakes, immersed in glycerine, a very minute quantity of tincture of iodine was added, scarcely sufficient to color the solution. After this addition, the cell walls nearly all disappeared, and instead of columns of polygonal cells, appeared masses of very distinct nuclei. The mucous membrane from which these flakes had exfoliated, was in many places diminished to half or a quarter its normal thickness. Empty spaces showed where gland cells, or gland tubes, had been. The epithelium was everywhere absent, but in most specimens remained a narrow, irregular border, deeply colored by carmine, and seeming to represent the round cells at the base of the epithelium. In one specimen only the highly colored nuclei of these cells remained, the walls seemed to have been dissolved away. The gland cells were all much larger than normal, the nuclei distinct and intensely colored by carmine, the cells in many places seeming to ascend from their places. On one specimen the lower part of some of the tubes was filled with fine detritus. None of the cells were, however, in the least colored, nor was there the least brown or gray tint over any part of the preparation. Whether or no the epithelium had been colored, but washed away, was impossible to ascertain. But as there was no epithelium in the flakes that covered the mucous membrane, it was hardly to be looked for underneath them. It had apparently

been completely destroyed. This difference cannot be ascribed merely to the dilution of the silver salt, which was one and two-third per cent. (one grain to sixty grains distilled water), a good deal stronger, therefore, than that with which the stomach from the dead rabbit had been treated. Yet in this case a dense brown precipitate was formed in and around the epithelium, which, far from exfoliating, was cemented more firmly in place, and the effects of the nitrate were never transmitted beyond the epithelium to the gland cells. The coloration of the cells seemed due to simple imbibition. But on the living subject, the introduction by endosmosis of the foreign substance, excited nutritive reactions; through the same endosmosis of living cells, the irritant was transmitted much further than was possible by the imbibition in dead or dying cells. There must have been a greater afflux of blood to the stomach under the influence of this extensive glandular irritation, although no traces of hyperæmia remained after death. Finally the contact of the silver solution with the mucous membrane must have been many times repeated. Few conclusions can be drawn from these few observations. Yet the following may, perhaps, be justified:

1st. The action of nitrate of silver upon living tissues is different from that exercised upon tissues whose vital connections have been severed, however recently.

2d. In the latter case an extremely weak solution gives a brown precipitate at the edges of the epithelial cells, and probably between them. This precipitate is formed almost instantaneously. If the solution be stronger, or the contact a little prolonged, the entire cell is colored, except the nucleus, This is a proof that the coloration depends on passive imbibition. and not on the vital activity of the cell. The relation of the silver salts to the nucleus seems to be exactly opposite to that of carmine. The weak solutions color the entire epithelium, but leave the glandular tissue untouched. A rather concentrated solution produces a precipitate only in the superficial layers of epithelium. The coloration of these layers, however, was not more intense with a strong solution than with one sixteen times weaker. It is possible, however, that stratified pavement epithelium offers more resistance to the imbibition than do single rows of columnar epithelium.

3d. The single application of a strong solution of nitrate of sil-

ver to a living epithelial surface produces the same effect as the more prolonged contact of a weaker solution, with membrane just removed from the body. In both cases the silver forms a superficial deposit, not always extending throughout the epithelial layers, never beneath them.

4th. On the living membrane a very weak solution, whose contact is prolonged, or frequently repeated, causes the deposit of silver in or around the epithelium, which facilitates its exfoliation, and consequent destruction. At least, this has been shown to be the case with the columnar epithelium of the stomach.

5th. The subepithelial glandular and connective tissue is irritated coincidently with, or consecutively to, this exfoliation. The irritation of the gland elements assumes the form characteristic of acute desquamative catarrh. There is cloudy swelling, increased size, and more intense carmine coloration of nuclei; loosening and even complete separation of cells, singly, or agglutinated in casts. The casts of the peptic glands in the specimen are strictly analogous to those shed from the uriniferous tubes, when their epithelium has been submitted to the influence of some irritating substance eliminated through them.

6th. The agglutination together of the cells that separated so easily from the wall of the tube, would seem to imply that the silver was first deposited at this latter point, surrounding the gland cells as it had the epithelium.

7th. It is noticeable that the walls of these cells, preserved in glycerine, were almost instantaneously dissolved by the addition of a very minute quantity of iodine.

8th. Coagulation of free mucus by the nitrate seems to play a very small part in its action on mucous membranes.

9th. Only microscopical examination of such apparent mucus can decide its real nature. In the case cited, and probably in many others, the so-called mucus consisted entirely of cells, coming not only from the epithelium (which had disappeared), but from sub-epithelial glands.

10th. From what precedes, it may be inferred that in therapeutical applications a weak solution of nitrate whose contact was prolonged, should exercise a more powerful and extended influence upon tissues than a strong solution applied once and immediately decomposed. In all catarrhal affections of mucous

membranes it is desirable, if possible; 1st, to remove proliferated epithelium: 2nd, to remove and provide for the healthy renewal of the diseased cell elements of glands. For both these purposes the first method should be more efficacious than the second.

The effect on blood vessels has not here been studied.

SPHYGMOGRAPHIC EXPERIMENTS UPON A HUMAN BRAIN, EXPOSED BY AN OPENING IN THE CRANIUM.[1]

Josie Nolan, aged ten, a very healthy Irish boy, had, eighteen months previous to observation, fallen and fractured his skull in the right fronto-parietal region. According to the mother's account, he remained insensible for two hours; but recovered consciousness about two hours after the fragments of broken bone had been removed by the trepan. The mother insists that from that time the wound healed rapidly, and that the child presented no morbid symptoms, not even fever. The history is evidently imperfect. At present there is an opening in the cranial bones, 2½ inches in the long diameter, 1½ inches transversely. The opening is situated in the right fronto-parietal regions, about 2 inches distant from the sagittal suture, towards which the long diameter is inclined at an acute angle. The opening is covered by a membrane, much thicker at the sides near the bones than in the middle. It is to be presumed that the central portion consists exclusively of dura mater, which, near the bony margin, is thickened by the addition of the remains of periosteum. The centre of this membranous covering is habitually somewhat depressed below the level of the cranial bones, but rises and falls in regular pulsations synchronous with those of the radial artery. Ordinarily, the effect of respiration is only distinctly seen in the sphygmographic trace; but, on forced inspiration, the membranes are clearly seen to descend still further below the level of the bones, and on forced expiration to bulge above it. Pressure upon the brain through these membranes causes no appreciable effect even on the pulse, and the boy, who has all the activity of his age, has, so far, never experienced the least inconvenience from

[1] Reprinted from the *American Journal of the Medical Sciences*, 1878.

this partial exposure of the brain. Under no circumstances, of digestion, exercise, or the influence of the various drugs administered during the experiment, was any change noticed in the colours of the membranes indicating increased vascularity in them. After exercise, they sometimes are bulging, but not always, and the effect of a temporary exertion rapidly disappears. When the boy is in a recumbent position, the level of the membranes is always higher than during the vertical position, whatever the level in the latter might be, or from whatever cause it had been effected.[1]

The case offered a unique opportunity for the study of conditions affecting intra-cranial pressure. For this purpose, Mahomed's sphygmograph was adjusted to the head of the boy, in such a manner that the lever pad rested on the thin central portions of the membranes, the rest upon the bones, and steadied by an assistant. The adjustment was always made with the boy in a recumbent position, the head but slightly elevated upon a pillow.

Before interpreting the traces, it is necessary to notice in what respects these must be expected to differ from those obtained from the expansion of an artery. It is obvious that the pulsating encephalon in our case differs from the pulsating artery: 1st, by its greater proximity to the heart; 2d, by its vertical position over the heart; 3d, by the immensely greater surface receiving the shock of the cardiac systole, and through which must be disseminated the tidal wave of blood; 4th, by the greater volume of blood thrown against this surface; 5th, by the greater freedom allowed to the excursion of the part of the brain exposed; 6th, by the greater slowness with which its mass could collapse upon the blood wave. The trace from the artery corresponds to the movement of the entire mass of fluid contained in it. But while the pulsations of the encephalon are due exclusively to the influx of arterial blood, this fluid is only one of three which are moving simultaneously in the pulsating mass, the others being the venous blood and the cephalo-rachidian fluid. 7th, the final difference to be noticed in the much greater influence of respiration upon the amount

[1] Since writing this paper I have seen an article in the *Centralblatt* for 1877, describing analogous experiments upon a woman's brain exposed by carcinoma. The experiments did not test the influence of drugs; but the conclusions so far as regards the normal movements of the brain agree with mine. See *Centralblatt*, Mai 12, 1877. Giacomini u. Masso, Beweg. des Gehirns.

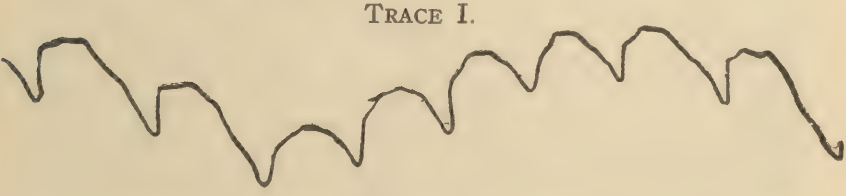
of blood contained at a given moment in the brain, as compared with that contained at the same moment in the radial artery.

These various circumstances will each have a specific effect upon the sphygmographic trace. Thus, the first five peculiarities enumerated will combine to give a much greater amplitude to the curve, or an immense increase in the height of the ascension line.

Owing to the fourth circumstance, the height of the tidal wave above the base of the percussion stroke will be greater; for, according to Mahomed, "this height indicates the amount of blood forced into the arterial system at each ventricular systole."[1] From the sixth peculiarity, the tidal wave should be more sustained. On account of the third character, there should be few oscillations from secondary waves; thus, dicrotic and elasticity oscillations should be little marked. On the other hand, the multiplication of resistances offered in the brain by fulness of its veins, or tonic contraction of its arteries, should render obliquity of the percussion stroke, and even anacrotismus of the ascending line more frequent. Finally, from (seventh) the greater influence upon intra-cranial circulation exercised by the aspirating force of inspiration, a much greater depression should occur at the moment of inspiration in the *ligne d'ensemble*.

The foregoing characters are all exhibited by the traces. The encephalic expansions, as uninfluenced by medicines, are shown in Trace No. I.; also, Trace No. VI. before the administration of atropia, and No. X. before coffee, and under the double influence of exercise and the digestion of a full meal.

TRACE I.



Under pressure 5.

Description.—Trace No. I. exhibits a peculiarity not observable in Traces VI. and X.; it possesses an anacrotic elevation, or an elevation on the ascending line.[2]

[1] *Med. Times and Gaz.*, vol. i., 1872, p. 129.

[2] Elevation first studied experimentally by Landois. *Die Lehre vom arterien Puls*, Berlin, 1872.

This is described by Mendel[1] as the character of the "pulsus tardus." In his schema, Landois succeeded in producing "anacrotismus" under one of three conditions, namely, when the exit opening of the schematic artery is narrowed; when the elasticity of its walls is diminished; and when, from increased volume of its contents, the internal tension is increased. Each of these conditions renders the distension of the tube by the systolic wave more difficult, hence prolongs the period of distension. Eulenberg shows that an anacrotic elevation may be obtained by compression of the artery beyond the point at which the sphygmograph is applied.

The other characters of this trace are, the well-developed tidal wave, or curve intervening between the percussion stroke and the aortic notch, and which, according to Mahomed, indicates the mass which has been thrown into the arteries by the cardiac systole; 2d, the deep inspiratory depression; 3d, the dicrotic elevation is slight, but more marked than in other traces.

Interpretation.—These characters, together with the short but vertical percussion stroke, indicate increased cerebral resistance with a large volume of blood in active circulation. The larger the mass to be aspired into the thorax at inspiration, the more marked must be the depression in the line of cerebral expansions, or the *ligne d'ensemble* of the trace.[2] The slightly increased dicrotism in the trace is to be referred to the state of the membranes, which were depressed, and flaccid, not tense or bulging. It was clear, therefore, that, notwithstanding the considerable tidal wave, the brain was not at the time distended. In another trace, taken when the membranes were tense and bulging, dicrotism had entirely disappeared. It is to be inferred that the tonic resistance of the blood-vessels was at this time great. Such a condition would at once explain the great resistance offered to the cardiac systole, causing anacrotismus, and the diminished tension of the membranes, permitting slight dicrotismus. The

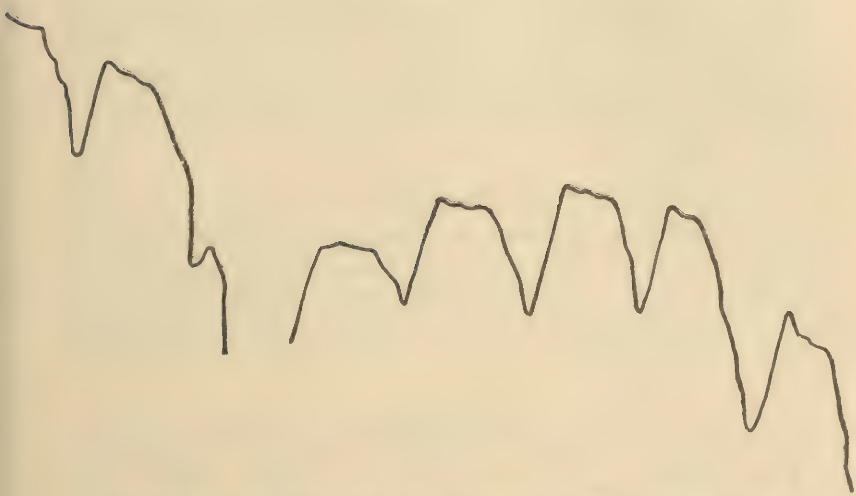
[1] *Arch. Virch.*, Bd. 66, p. 260. See also Eulenberg, *Arch. Virch.*, Bd. 45, 1869.

[2] The percussion stroke is shorter during inspiration than during expiration. Since at this moment the cerebral resistance is diminished, this shortening must be due, not to increased resistance, but to diminished force of the heart. This diminution is caused by the "negative pressure" exercised on the heart during the expansion of the thorax, and thus is secured a real intermittence in the blood-pressure to which the brain is subjected.

radial pulse showed high tension, and complete absence of dicrotism.

Hence, important corollary, we must conclude *that intra-cranial pressure* (such as would distend the membranes) *is not necessarily in proportion to the tension of the cerebral blood-vessels, or to the height of their tidal wave, but may be just the reverse.*

TRACE II.



Pressure 5, two hours after 5 grs. of sulphate of quinia. Pulse 90.

Description.—Trace No. II. may be described as follows: Percussion stroke perfectly vertical and very high (by exact measurement one-third higher than in Trace No. X., the next highest observed). The angle between the percussion stroke and the line of descent of the preceding curve is very acute. Entire absence of anacrotismus. The systolic apex forms an acute angle, and is followed, not by a rounded curve, but by a horizontal, even slightly concave line. The tidal wave is very small. The line of descent is abrupt, and the dicrotic elevation very near to its terminus. Finally, the inspiratory depression in the *ligne d'ensemble* is enormous.

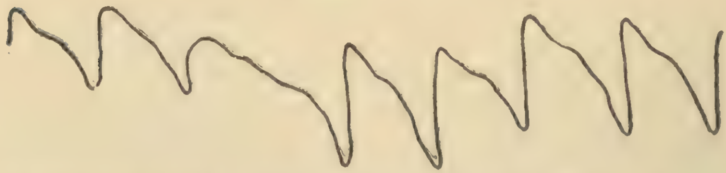
The membranes bulged more at each cardiac systole than before the administration of the quinia, but were not tense.

Interpretation.—The height and vertical direction of the percussion stroke are not exclusively due to increased energy of the

cardiac systole, since when this is obtained by brandy the percussion stroke is much lower (see Trace No. IV.). Hence, in addition to the effect on the heart, there must be diminution of the intra-cranial resistance. The acute angle of the systolic apex implies an instantaneous momentary collapse of the cerebral blood-vessels after their distension by the percussion stroke. From the smallness of the tidal wave we must conclude that little blood is retained in the arteries at any given time. But the prolonged horizontal line between the systolic apex and the summit of the tidal wave, implies a sustained tension of the arterial walls. The line resembles that observed in traces from atheromatous arteries. But the abrupt line of descent indicates powerful elastic contraction of the arteries, contrary to what is seen in atheroma.

Conclusion.—By a tonic dose of quinia, the energy of the cardiac systole is increased; the tonus and elasticity of the walls of cerebral blood-vessels are also increased, so that the blood is forced rapidly on through the capillaries, thus diminishing the resistance to the cardiac systole. More blood is admitted to the brain, but the intra-cranial pressure is lessened.

TRACE III.



Two hours after 20 grs. of quinia. Pressure 5. Pulse 96. Temperature fallen one degree. Membranes depressed.

Description of Trace III.—Percussion stroke vertical, but shorter than in Trace II. Systolic apex angle acute, and followed by descending instead of horizontal line. Tidal wave unequally developed, in some curves almost absent, in all very small, and far below the level of systolic apex.

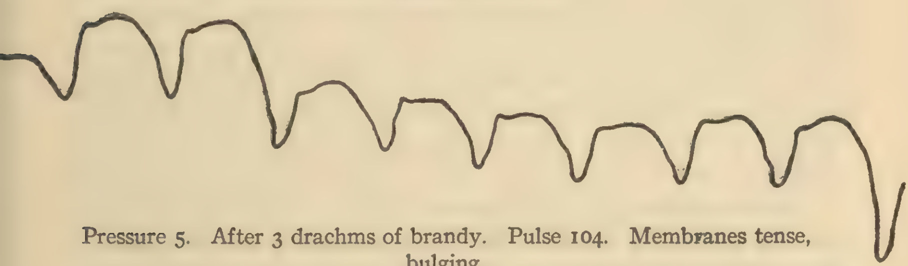
Interpretation.—Diminished intra-cranial resistance to percussion stroke; nevertheless, small amount of blood thrown into brain, rapid and complete collapse of cerebral arteries.

Conclusion.—Diminished energy of cardiac contractions, unfilled cerebral arteries, great diminution in intra-cranial pressure.

It is important to notice that the radial pulse taken at this time exhibited a relatively much larger tidal wave and higher tension than was shown by these cerebral traces. We should infer therefore *that the diminution of intra-cranial pressure was out of proportion to the general diminution of pressure in the arterial system connected with sedation of the heart.*

Description of Trace IV.—Percussion stroke not quite vertical, much shorter than after quinia; systolic apex forming a right, instead of an acute angle; tidal wave greatly developed; line of descent oblique and gradual; angle between it and the following percussion stroke rather wide; dicrotism scarcely perceptible; inspiratory depressions not very marked, and much prolonged, comprising four curves, while the period of expiration comprises three.

TRACE IV.



Pressure 5. After 3 drachms of brandy. Pulse 104. Membranes tense, bulging.

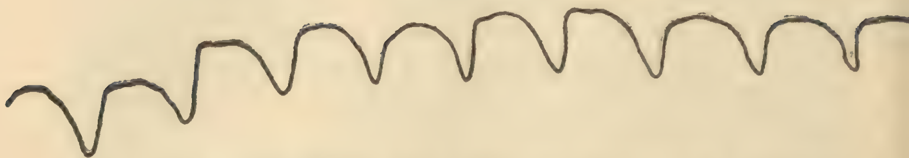
The membranes were tense, bulging, and affected by a peculiar heaving pulsation, not seen in any other case; the pulse was 104.

Interpretation.—Increased mass of blood in brain; increased resistance to percussion stroke dependent on this, and less than that which would be associated with contracted arteries; [1] (see Trace I.) slow collapse of arterial walls, notwithstanding rapid circulation; increased duration of inspiration; slow aspiration of blood from brain.

Conclusion is mainly expressed in the interpretation. The increased force of the heart is indicated by the radial pulse; its effect on the brain as shown in the trace, is partially compensated by the *increased intra-cranial resistance. The cerebral blood-vessels are dilated, implying diminished tonus of their walls; the intra-cranial pressure increased.*

[1] Hence the percussion stroke, though short, is not anacrotic.

TRACE V.



After 5 gtts. tincture belladonna ter in die for four days, and 5 gtts. every three hours on fifth day. Pulse 108. Pupils moderately dilated, membranes bulging, not tense in recumbent position.

Description of Trace V.—General resemblance to Trace IV. under brandy. Percussion stroke one-fifth higher than in Trace IV.; systolic apex a right or slightly obtuse angle; tidal wave developed about as much as with the brandy; line of descent gradual, without dicrotism; absence of inspiratory depression; rise of entire *ligne d'ensemble*, as if from prolonged expiratory effort. All the characteristics of the trace were developed under a pressure of four ounces, as was not the case with brandy; but the percussion stroke was then higher than is represented in Trace V. The membranes did not bulge at all when the boy was vertical.

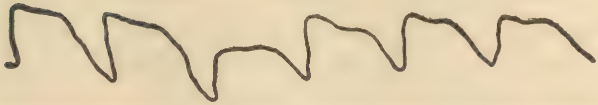
Interpretation and Conclusions.—Mass of blood in the brain increased about the same as after brandy; but intra-cranial pressure less (as shown by condition of membranes, and response to lower pressure of sphygmograph). *Expiration prolonged.*

Remarks.—From the traces alone it is rather difficult to understand why the tension of the membranes should have been so great with the brandy, and so slight with the belladonna; the rapidity of the circulation was almost the same in the two cases (pulse 104 and 108). The difference probably depends on accelerated capillary circulation in the case of belladonna, and retard of the same after brandy.

TRACE VI.

Before atropia, membranes depressed.

TRACE VII.



30 minutes after, $\frac{1}{64}$ gr. atropia, subcutaneously. Pulse 120.

Description.—Trace VII. Half an hour after $\frac{1}{64}$ gr. atropia shows, as compared with Trace VI., taken just before; that the percussion stroke is double the height, and more nearly vertical; the anacrotism has disappeared; the angle of the systolic apex rounded, but followed by descending instead of ascending line; tidal wave much diminished; dicrotic elevation increased, and nearer by one-fifth to the percussion stroke—that is, the duration of the ventricular systole is one-fifth less. Inspiratory depression remains the same, slightly marked, and comprising a single curve. The membranes were raised, but neither tense nor bulging. The radial pulse had become dicrotic.

Interpretation.—Relaxation of cerebral blood-vessels; consequent diminished intra-cranial resistance to percussion stroke; more rapid collapse of arterial walls; diminution in mass of blood retained in brain.

Conclusion.—Diminution of intra-cranial pressure, but increased amount of blood passing through brain in given time; on account of accelerated cardiac action and diminished resistance to it.

Description.—The peculiar effect produced by the drug is not perceptible in any individual trace alone, but in a comparison between the traces taken under moderate pressure (four and five ounces, Trace IX.), or under higher pressure (six ounces, Trace VIII.). In this the ascending stroke is anacrotic, in the others not. The tidal wave is also much less developed.

TRACE VIII.

Pressure 6.

Interpretation.—The increase in pressure of the sphygmograph lever is transmitted to the cerebral arteries, so as to offer

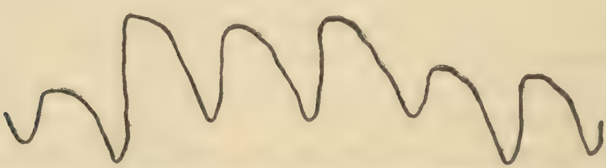
decidedly increased resistance to the ventricular systole, and instead of developing the percussion stroke, breaks it. That such slight increase of pressure is able to cause anacrotismus, shows that *the force of this systole, i. e., of the heart's action, has been*

TRACE IX.

2½ hours after ¼ gr. tartar emetic. No vomiting. Membranes apparently tense, bulging. Pulse 112. Pressure 5.

weakened relatively; *that the intra-cranial pressure is not only diminished*, but is easily overcome by external pressure; in other words, that the walls of the arteries are relaxed. This peculiarity is not observed in any other trace, even that of the sedative dose of quinia, but is confined to the nauseating dose of tartar emetic. After vomiting, the intra-cranial pressure is raised, and resists the higher pressure of the sphygmograph.

TRACE X.



Pressure 5. Before coffee, pulse 112. Membranes tense, bulging.

TRACE XI.

Pressure, 5. Half an hour after 4 oz. strong infusion coffee. Membranes much depressed. Pulse 112.

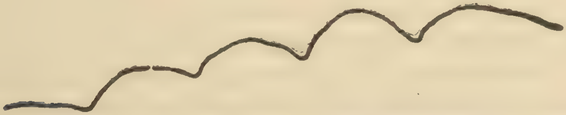
Description (Trace XI.).—Absence of inspiratory depression, which has been marked in Trace X. Percussion stroke shortened to one-fifth the height, oblique, instead of vertical; higher under

pressure 6 than 5. Diminution of tidal wave. The membranes were depressed, which had been bulging. The radial pulse remains the same in rapidity, and also in the form of the sphygmographic trace (not here given).

Interpretation.—From this last fact it is evident that the percussion stroke has not been shortened by weakening the force of the cardiac contraction. The shortening must, therefore, be due to an increased resistance in the brain. As there is not an increased mass of blood in the brain, the resistance implies increased tonicity—increased contraction of blood-vessels. This tonicity is only overcome by greater external pressure; hence percussion stroke is more developed under pressure 6 than 5 (reverse of tartar emetic).

Conclusion.—The amount of blood circulating in the brain is smaller, but it is brought to nerve tissues under increased pressure; hence assimilation of nutritive material should be increased in rapidity, if lessened in quantity. *The intra-cranial pressure, on the whole, i. e., against the membranes, is diminished.*

TRACE XII.



Pressure 4. Three hours after twenty grains of bromide of potassium.

TRACE XIII.

Pressure 5. Pulse 76; membranes depressed below cranial level.

Description.—Great development of tidal wave, perceptible under all pressures. At pressure 4, percussion stroke so oblique as to merge into tidal wave. Trace resembles that from an aneurismal tumour.[1] Under pressure 5, percussion stroke sometimes vertical, sometimes oblique. Line of descent prolonged and gradual, without trace of direction.

Interpretation.—The trace must be considered in connection

[1] See trace given by Mahomed, *Medical Times and Gazette*, 1873, p. 222.

with the facts, that the membranes had become depressed, and the tidal wave of the radial pulse extremely small, under the influence of the bromide. It is to be inferred, therefore, that the large tidal wave in the cerebral trace does not depend upon an unusual amount of blood thrown into, or contained in, the brain, but upon unusual obstacles to its passage out of the brain. This implies a *contraction of the smallest blood-vessels and capillaries, the larger remaining the same, and thus offering no other obstacle to the ventricular systole than the prolonged retention of blood in them; the latter causing increased lateral pressure, identical with that of a large tidal wave.*

Conclusion.—The intra-cranial pressure, on the whole, i. e., against the membranes, is diminished; but the brain tissue is subjected to a mechanical pressure from fulness of the vascular canals before the point where they begin to be nutritive, and because of relative exclusion of the blood from the latter.

Remarks.—The descriptions of the traces of coffee and bromide read a good deal alike, except in regard to the tidal wave; but the traces are conspicuously different. The difference probably depends on the different rate of the circulation, on the different direct action of the drugs on the nerve tissues, and on the exercise of lateral pressure in the *nutritive* blood-vessels in the case of the coffee; in the canals leading to them, in the case of the bromide. In the case of the brandy an increased tidal wave was interpreted as evidence of dilatation of cerebral blood-vessels, because of the visible increase in the tension of the cerebral membranes and the state of the radial pulse which coexisted.

The characteristic trace of the bromide was not developed until three hours afters its administration. It was most characteristic at a low pressure (4). It is not believed that the whole, or even the greater part of the physiological action of bromide of potassium can be explained by this effect upon the cerebral blood-vessels.

To what extent the conclusions, drawn from these observations, are in accordance with existing theories, may be considered on another occasion. On this, we content ourselves with registering the facts.

ACUTE FATTY DEGENERATION OF THE NEW-BORN.[1]

THE following case offers, we believe, an interesting illustration of a recognized, but still rare and not completely understood disease.

Early in the past year, Mrs. H. asked my advice under the following circumstances: She had been married twelve years, and her eldest and her only surviving child was then eleven years old. I have forgotten the fate of the second child. The third was born prematurely at seven months, and died within twenty-four hours after birth. The fourth was apparently healthy for three or four days, then began to have hemorrhages from the navel which resisted the application of iron styptics, and to whose repetition the child finally succumbed on the eighth day after birth. The fifth child was born dead at term—a week after the cessation of all movements. During this fifth pregnancy, Mrs. H.'s health, which previously had been irreproachable, had suffered a good deal, and she had noticed that her abdomen remained much smaller than in former pregnancies. The child at birth was said to have been very small, but no exact measurements were taken. After this confinement, Mrs. H. recovered her health completely. At no time, so far as could be ascertained by the history, did she present symptoms of endometritis, still less of syphilis. After the misfortune of the fifth confinement, she was advised, by a well-known German physician of this city, that, in the event of another pregnancy, she should watch the movements of the child carefully, and should they, near term, begin to grow weaker, that she should at once report the case to a physician, who might save the child by bringing on a premature confinement.

It was for precisely this exigency that Mrs. H. consulted me.

[1] Reprinted from the *American Journal of Obstetrics*, 1878.

About a year after the fifth confinement, she again became pregnant, and was now two weeks before term. During the period of gestation her health had been uninterruptedly good, and the movements of the child vigorous, until a few days previous, when they had begun to grow much weaker, and, warned by her previous experience, she was fearful that they might soon cease altogether.

Upon examination I found the child apparently well-developed, the head presenting in the first position, the amniotic liquor in sufficient abundance, but not excess. The uterine souffle was loud and distinct, but the most careful search failed to discover the fetal heart, although the spontaneous movements of the child showed that it was still alive. It was evident, therefore, that the heart's action had begun to flag, under some unknown morbid influence, probably identical with that which had caused the death of the third and the fifth child. It was evident, further, that if this lethal influence depended on any lesion of the fetal viscera, a premature confinement would rather hasten death than avert it; while, if it were due to some morbid condition of the placental or umbilical circulation, this expedient might possibly save the life of the child, as, in view of the previous history, there was so much reason to suppose that the child would die if left to itself. I determined to act on the hypothesis which afforded the only excuse for action or hope of safety, and, confirming the previous opinion, advised artificial delivery, to which the parents readily assented. The cervix was extremely soft and dilatable, and by means of Barnes' dilators, labor pains were induced in about twenty-four hours. The child (a girl) was born seven or eight hours later, and, although rather small, seemed fairly vigorous. It cried immediately after birth, and showed no sign of asphyxia. The cord was to external appearance healthy. The placenta adhered for nearly an hour, and then was removed by artificial detachment, and torn during the process. But that all fragments were entirely removed from the uterine cavity was demonstrated, if necessary, by the rapid and complete recovery of the mother.

In securing the cord, after the child had been washed by the nurse, I took the precaution to apply two ligatures, as tightly as possible. I then left the child in apparently a very satisfactory condition, but two hours later, four hours after birth, was recalled

by the tidings that an alarming hemorrhage had just taken place from the cord. The nurse compressed the cord with her fingers, and a neighboring physician, summoned while awaiting my arrival, found that this manœuvre had nearly arrested the hemorrhage just before he came in. The child, however, was perfectly blanched, and to prevent, if possible, a recurrence of the danger, the doctor wound an elastic ligature tightly around the cord, from its free end to its cutaneous surface, not encroaching upon the latter He confirmed the statement of the nurse, that the blood had been seen to ooze from a point above the upper ligature, just at the junction of the mucous and cutaneous surfaces of the cord. At this point a minute tear was perceptible.

I did not see the child until nearly an hour after the accident. By that time color had returned, there was sufficient warmth, and a few drops of brandy in water were readily swallowed. The child was left wrapped in cotton-wool, and perfect quiet enjoined. This was at noon.

At four o'clock in the afternoon, when recovery from the first accident seemed complete, I resolved to take the last precaution against its repetition, which seemed only too probable, by passing a couple of hare-lip pins at right angles to each other through the cutaneous base of the cord, and winding a ligature tightly around them, as in the strangulation of a nevus. The child was not moved from its cotton-wool nest during the little operation. Neither at this time nor later was there any disturbance of respiration. At six o'clock, it was doing very well; at ten, as I entered the room, the mother exclaimed with great satisfaction that the baby must be growing stronger, for it had been screaming loudly for half an hour, and its cries had only just ceased. The quiet was ominous, and on approaching the child I found that the face had again become perfectly white, and that it had ceased to cry because it had ceased to breathe. The heart still beat feebly, but in a few minutes its pulsations also ceased, and life was completely extinct. Upon removing the coverings, I found that a slight amount of oozing had again taken place at the navel. There was none, however, around the hare-lip pins. This slight hemorrhage seemed altogether insufficient to account for the death. The real cause of death was revealed at the autopsy made twenty-four hours later.

Autopsy

Abdomen.—Several teaspoonsfuls of fresh fluid blood were found in the peritoneal cavity, some lying on the surface of the intestines, more gravitated into the flanks. There was no hemorrhage of the intestine, either into its coats or into the cavity. No trace of peritonitis. The blood seemed to have come chiefly from a rupture of the umbilical vein, between the navel and the liver, for in the connective tissue surrounding it, about half an inch above the navel, was a blood-clot the size of a French bean.

No hemorrhages were discovered in the liver, spleen, or kidneys, but minute submucous hemorrhages were scattered through the stomach.

Thorax.—Here a most peculiar appearance was presented. The lungs were fully expanded, and collapsed imperfectly on the removal of the sternum. Scattered over their entire surface were numerous hemorrhagic spots, whose pleural area varied in diameter from a few lines to half an inch. The depth of these foci was generally about the eighth of an inch. The deep extremity was somewhat smaller than the superficial, but in no case did they offer the wedge-shape characteristic of embolic infarcti. Similar hemorrhagic spots were disseminated throughout the parenchyma of the lungs. Between these spots the lung-tissue was normal to the naked eye.

There was no effusion into the pleural cavity. There were no macroscopic alterations of the heart. The cranial cavity was not opened.

Microscopic Examination

Umbilical cord.—The umbilical vessels were examined externally to the abdomen, and also for a short distance above and below the internal surface of the navel. All the sections were normal.

Liver.—Examination of sections at a low power in no case succeeded in discovering the distinct separation into lobules, which is usually so easy to demonstrate on the liver of a new-born child. At the same power, the larger bile ducts were observed to be surrounded by tissue, colored bright blue by the hematoxyline, in striking contrast to the pale color of the rest of the section. A higher power resolved these blue bands into masses of

round nucleated cells. The liver-cells were filled with fine fat-granules, which in no case were aggregated into globules. The size, shape, and nucleus of the cells were preserved, the nucleus coloring deeply with hematoxyline.

Kidneys.—The epithelium was degenerated both in the cortex and in the medullary portion, but the process was much further advanced in the convoluted tubes than in the straight. In some of the tubes the epithelium was completely broken down, the nucleus gone in others, while the cells were full of fine granules; the nucleus was preserved and distinctly colored. The Malpighian glomeruli were generally intact, but in some cases the epithelium covering them was degenerated like that of the tubes. In some places, quite a large area of the microscopic section was so completely degenerated that it appeared a uniform pale-yellow color, no nuclei remaining to take up the color of the staining fluid. In other places again, a small number of the tubes would appear completely normal. There was no proliferation of epithelium, and the lumen of the tubes was nearly always free. The connective tissue was not increased. The blood-vessels showed a few fat-granules in the tunica media.

Stomach.—The sections of the stomach did not show any degeneration of the glandular cells, and were not sufficiently successful to demonstrate the condition of the cylindrical epithelium. On one section was found a mass of blood-corpuscles effused just at the base of the glands. The submucous blood-vessels showed the same sparse scattering of fine black granules between the nuclei of the muscular coat as were found in the renal vessels.

Lungs.—Blood-corpuscles were scattered in abundance throughout every section, sometimes aggregated into masses, when the section had passed through a focus of hemorrhage, sometimes sparsely disseminated. The capillaries all contained blood, and the loops of vessels which encircled the alveoli looked as if they had been artifically injected. In some cases, these loops almost entirely occluded the alveolus. There was also a great abundance of epithelium in various stages of fatty degeneration. In some cases the epithelium had broken down into fatty detritus. These appearances have been described in cases hitherto related. But one feature in this case, of which I have not found any previous mention, was the appearance of fat-granules in great abundance in the muscular coat of the arteries,

interspersed among the nuclei. These granules dissolved in ether, and resisted acetic acid.

In two sections were discovered the point of rupture of medium-sized blood-vessels, with a stream of blood-corpuscles pouring through.

Heart.—The muscular fibres of the heart contained fat-granules, but in excessive abundance.

The case above described was evidently one of the rare disease first described by Hecker and Buhl in 1861, under the name of "acute fatty degeneration of the new-born." Buhl's description[1] is as follows: "The lungs contain smaller or greater, tolerably circumscribed hemorrhagic infarcti; the bronchi contain pure blood or bloody mucus. The parenchyma of the lung is somewhat edematous; the pavement epithelium filled with fat-granules. There is, further, fatty degeneration of the muscular fibre of the heart, and of the epithelium of the kidneys and liver. As a consequence of the pulmonary and cardiac lesions, the child soon shows symptoms of asphyxia and cyanosis; and as a consequence of the hepatic degeneration becomes icteric." Buhl attributes the multiple hemorrhages to alterations in the composition of the blood and the texture of the blood-vessels, both probably dependent upon the "acute nephritis and hepatitis."

In the *Arch. für Gynaek.*, Bd. x., Hecker describes a new case of this disease. The child died in fourteen hours, after a slight hemorrhage from the navel quite insufficient to explain the catastrophe. The icterus was present at birth, and intense. At the autopsy were found numerous subpleural ecchymoses, large wedge-shaped pulmonary infarcti, fragility of walls of pulmonary vessels. There was blood in the pericardium, stomach, and intestine; the liver was pale and fatty; the heart soft and fragile; the spleen enlarged. In another case, reported by the same author, the blood was leukemic. Hecker remarked that this disease is so rare that it has scarcely been mentioned in literature since Buhl's first description of it was given. Müller in a chapter devoted to acute fatty degeneration of the new-born in Gerhardt's Cyclopedia,[2] only quotes five articles on the subject; of these only two[3] describe the disease in the human

[1] *Klinik für Geburtskunde*, 1861.
[2] Bd. ii., 1877.
[3] Hecker u. Buhl, loc. cit., and Hecker, *Monats. für Geburtskunde*, Bd. xxix.

subject. Furstenberg[1] describes an analogous affection in the new-born of domestic animals. Roloff writes about young pigs[2] and foals.[3] The latter case is really an acute malignant osteitis, and has but very slight resemblance with the morbid condition which occupies us. Müller himself contributes nothing original to the subject.

Three symptoms are prominent in the clinical history of this disease: umbilical hemorrhage, cyanosis, icterus; the latter constantly increasing until the moment of fatal termination. Of these, the first, or umbilical hemorrhage, was alone present in our case. It occurred earlier (two hours after birth) than in any case of which I can find a record. According to Hennig, of all cases of omphalorrhagia, the greatest number occur on the seventh day, that is, at or after the fall of the cord. He only counts 7 cases out of 135, as occurring on the first day. According to Bouchut, the greatest frequency is from the third to the ninth day. Tanner only mentions the cases which occur after the fall of the cord. Although the accident is itself infrequent (Hennig reckons it as occurring once in 5,000 children), only a certain proportion of cases are to be attributed to generalized fatty degeneration. The belief that the hemorrhage principally depends upon imperfect ligature of the cord is widely diffused, both among the laity and in the profession, and has been not infrequently the basis of suits for malpractice. Thus Cripps Lawrence[4] declares that early hemorrhage is from the funis, and easily controlled by prompt ligation, even when the blood comes from the *side* of the cord. In the latter case, the accoucheur is innocent of carelessness; but the ordinary ligature must be complemented by another on the ventral side of the bleeding point. "With this exception," asserts the writer, "omphalorrhagia preceding the fall of the cord is always due to imperfect ligature, or to imperfection in the material used for ligature, or to improper handling of the ligatured end of the cord."

Mr. Lawrence admits that hemorrhage from the umbilicus, which he calls secondary, is a serious and "sometimes" fatal accident. He fails, however, to perceive that this hemorrhage may occur before, as well as after, the fall of the cord. Braun, in 1871, had pointed out that the main significance of umbilical

[1] Virch. *Arch.*, Bd. xxix. [2] *Ibid.*, Bd. xxxiii. [3] *Ibid.*, Bd. xliii.
[4] *Obstetrical Journal*, vol. iii., 1875.

hemorrhage depended, not on the epoch at which it occurred, but on its origin from the umbilical vessels themselves, or from the "parenchymatous" vessels supplying the navel. The latter always depends on constitutional disease. Similarly Vogel describes the blood "welling up from the umbilical depression" left after the fall of the eschar. We have said that in our case, where the first hemorrhage took place two hours after birth, the blood was seen to issue from a minute opening at the junction of the mucous and the cutaneous surfaces of the cord, hence must be classified with those cases called by Braun "parenchymatous."

The constitutional disease to which such parenchymatous hemorrhage has often been attributed is hemophilia. This has sometimes been inferred merely from the repetition of the accident in several children of the same family. Thus Jenkins[1] remarks that 17 mothers, among those whose histories he had collected, lost more than one child by umbilical hemorrhage. The same repetition of the accident was remarked in several children of Mrs. H. (our case). Hennig, however, observes that the tendency to multiple hemorrhages, characteristic of these cases, bears much more analogy to scorbutus than to hemophilia. Grandidier[2] declares that navel hemorrhage is only to the smallest extent the expression of the hemophilic diathesis. It is not especially frequent in families of "bleeders," and the children who survive the accident do not manifest hemorrhagic tendencies in later life. Out of 228 cases of omphalorrhagia, only 14 come from "bleeders," belonging to 11 hemophilic families. The bleeding diathesis is 13 times more frequent in males, but omphalorrhagia is more frequent in female children. The evidence seems conclusive that the accident of navel hemorrhage is not, certainly in the great majority of cases, produced under the influence of the constitutional disease with which it seems most naturally affiliated.

When the umbilical hemorrhage has been preceded by icterus, which gradually deepens coincidently with the repetition of the bleeding, there is no hesitation felt in referring the accident to the acute fatty degeneration which has been considered by Buhl as identical with the malignant icterus of adults. The

[1] *Report on Spont. Umbil. Hem.*, 1858.

[2] *Die freiwillig. Nabelblut.* Cassel, 1871.

hemorrhage is then generally considered to be a consequence of the icterus; as in acute liver atrophy of adults, the blood is supposed to be poisoned, "dissolved" by biliary acids absorbed from the degenerated liver-cells, and on this account to transude readily the walls of its containing vessels.

The autopsy in our case, however, demonstrated: 1st, that all the lesions proper to Buhl's disease may exist without the occurrence of icterus at any time before death. And 2d, that, in the absence of icterus, hemorrhages may occur—both umbilical and visceral—and in sufficient abundance to prove fatal. It is evident, therefore, that icterus is neither necessary to the diagnosis of the disease, nor to the production of its most dangerous symptom.

The icterus is in proportion to the degree of degeneration of the hepatic cells. In our case, although these cells were filled with fat-granules, their contour and nuclei were intact, none had fallen into the detritus characteristic of the advanced stages of acute hepatitis. We must evidently conclude that, in this case, death occurred at a comparatively early stage of the disease.

If the hemorrhage can be thus independent of the icterus, the sign of blood-poisoning, there is little reason for admitting, with Buhl, that it, or even the generalized fatty degenerations, result from the action of a special poison in the blood. The multiplicity of the hemorrhages indicates a cause for them that must have been generalized throughout all the tissues and organs of the body.

Many morbid conditions are known to be characterized by multiple hemorrhages. Besides scurvy, hemophilia, and malignant icterus already alluded to, it is well known that in yellow fever and phosphorous poisoning, with their remarkable analogies to the last disease, and also in pernicious anemia, which in many respects approaches the first, visceral hemorrhages are as frequent, or even essential to the complete evolution of the morbid process.

It is remarkable that anatomical alterations of the walls of blood-vessels have rarely been demonstrated in these hemorrhagic diseases. In scurvy, Krebel[1] imagines that the small blood-vessels are partially paralyzed, a gratuitous assumption,

[1] *Der Scorbut*, 1862, S. 190.

and which would not really explain the hemorrhages. In hemophilia, many investigators have found no alterations of the blood-vessels, although Legg and Grandidier admit that, in a number of cases, their walls have been found abnormally thin. Immerman[1] says: "We must accept as an anatomical substratum [of this disease], as an anomaly not exclusive to chlorosis, a general hyperplasia of the arterial vascular system, as also the partial fatty alteration of the walls of the blood-vessels which usually accompanies it."

In regard to yellow fever, Haemisch[2] observes that, when the red corpuscles are destroyed, the blood decomposed [under the influence of the fever poison], the blood loses its power to nourish tissues [including the walls of vessels]; hence their great fragility and consequent rupture. Leyden[3] observes that fatty degenerations are produced by all poisons which destroy blood-corpuscles. When the ductus choledochos was tied, and the absorption of bile necessitated by obstruction to its passage, icterus was first induced, then multiple ecchymoses. "The action of altered blood on a part [in producing fatty degeneration] is analogous to the effect of exclusion of blood by means of an embolus or a ligature."

The majority of the writers on phosphorous poisoning refer the characteristic hemorrhages directly to the alteration of the blood, which alone is sufficient to cause transudation.[4] But Klebs[5] found in the adventitia of the small vessels, both veins and arteries, abundance of fine granules, partly albuminous, partly fatty. There was no alteration of the vessels of the brain, and this organ alone was exempt from hemorrhage. "The alteration of the vessels," remarks Klebs, "is the necessary intermediate lesion between the alteration of the blood and the production of extravasation. The latter never result from coarser ruptures of continuity of the vascular walls."

On the other hand, as is well known, Cohnheim[6] has shown that even a brief interruption to the nutrition of the walls of blood-vessels will so injure their integrity that their pores will

[1] Ziemssen's *Cycloped.*, art. Hemophilia, Bd. xii. [2] *Ibid.*, Bd. ii.

[3] *Der Icterus*, p. 180.

[4] See Lewin, Virch. *Arch.*, Bd. xxi. Bernhardt, *ibid.*, Bd. xxix. Wegner, *ibid.*, Bd. xl. Baunier, *Gaz. Med.*, 1868. Lebert et Wyss, *Arch. Gen.*, 1868.

[5] Virch. *Arch.*, Bd. xxxiii. [6] *Ueber die Embol. Processe*, 1872.

admit the transudation of red blood-corpuscles.[1] Cohnheim has further demonstrated, that, not only interruption of the circulation by ligature, but also venous stagnation will suffice to produce the deterioration.[2] Now, in the lungs of our case was abundant evidence of extensive venous stagnation; the small blood-vessels and the capillaries were choked with blood. The cause of this arrest of the pulmonary circulation lay in the weakened force of the partially degenerated heart. In other cases on record, fatty degeneration of the cardiac muscles has been much more extensive than was noted in ours; and, perhaps correlatively, asphyxia has been a prominent symptom, while in our case it was entirely absent. But the flagging of the heart's action was shown even before birth, by the impossibility of discovering its sounds on auscultation.

The hemorrhages in the lungs, the most extensive and remarkable revealed by the autopsy, were therefore explicable by one or more of several different lesions. 1st, The fatty infiltration of the walls of the blood-vessels. 2d, Nutritive alteration of these walls, not demonstrable to the microscope, and dependent upon, (*a*) acute poisoning of the blood; or on (*b*) intensely anemic impoverishment of the blood; or (*c*) on the venous congestion, itself caused by failure in the contractile force of the heart.

What is the immediate cause of the fatty degeneration of the heart, of the blood-vessels, the epitheliums, and the gland cells, which underlies the hemorrhage and all other clinical symptoms of the disease?

As already noticed, Buhl assimilates the disease to malignant icterus, and considers that an acute inflammation sets in shortly before birth and rapidly fulfils its course afterwards.

For obvious reasons, the special clinical symptom of such an inflammation, namely fever, cannot be ascertained at the time the disease is presumed to commence. But fever has not been noticed after birth, in the cases of infants dying from acute fatty degeneration. To us, the facts in regard to the multiple fatty degenerations all point, not in the direction of inflamma-

[1] Concerning the precise mechanism of this extravasation, and ingenious hypotheses on the nature of the pores, see Schklarewsky, Pflug. *Archiv.* Bd. i. (Ueber Diapedese.)

[2] Virch. *Archiv.*, Bd. xli., S. 220.

tion, but of direct arrest of nutrition, by arrest of vascular supply.

That diminution of the quantity, as well as alteration in the quality, of blood supplied to a tissue may induce fatty degeneration in it is well known. Several recent experiments set this part in a very clear light. Zielonko[1] introduced two complex conditions into his experiment, when he gradually narrowed the entire aorta, and observed the effect on the kidneys. According to the degree of narrowing, he found albuminous tumefaction of the kidney, moderate hydronephrosis, and fatty degeneration of the epithelium. But in this case the rise of tension in the venous system must have been great enough to account for the results, as much as the arterial anemia.

But Perl[2] imitated pathological conditions better, when he subjected dogs to repeated venesections, at intervals of five days. When three per cent of the body weight was abstracted by the bleeding, fatty degeneration of the heart was always produced.

Von Platen[3] gradually compressed the renal artery by means of a silver clamp. This interruption to the circulation of the kidney was followed by various degrees of fatty degeneration of their epithelium. If only a single branch of the renal artery was compressed, the degeneration was limited to the territory supplied by it.

Recklinghausen[4] describes a cloak ("*Mantel*") of fatty infiltration around a hemorrhagic infarctus of the kidney. He observes that, after ligature or embolus of one branch of a renal artery, there will be necrosis in the centre of a territory supplied by it; but fatty infiltration on the periphery, where an imperfect nutrition is maintained by means of collateral blood-vessels.

So far as I know, Pepper is the only writer on pernicious anemia, who considers it as other than a primary blood-disease, characterized by rapid diminution in the mass of the blood, and in the number of its red corpuscles. Pepper, however, in a single autopsy, found an alteration in the medulla of the bones, resembling that described by Neumann in medullary leukemia;

[1] Virch. *Arch.*, Bd. lxi., S. 267.
[2] Virch. *Arch.*, Bd. lix.
[3] Virch. *Arch.*, Bd. lxxi., Heft I.
[4] Virch. *Arch.*, Bd. xx., S. 205.

and on the strength of this observation, this author ranks pernicious anemia as a form of leukemic disease.[1]

If, for the reason alleged in the footnote, we set aside this opinion, we should find all observers agreed in considering this "idiopathic anemia," and the fatty degenerations characteristic of it, as an exquisite clinical counterpart to the pathological experiments already quoted: to the experiments of Cohnheim and Arnold, showing the influence of denutrition of the walls of blood-vessels upon the production of hemorrhages; to those of Ponfick, Perl, Platen, and others, showing the influence of diminished afflux of blood in the production of fatty degenerations or infiltrations.

"The general mass of blood is diminished; there is true oligemia; multiple hemorrhages are frequent; those into the retina almost characteristic. We shall not err if we attribute this development of a hemorrhagic diathesis to an alteration of the walls of blood-vessels, caused by the alteration in the quantity and the quality of the blood. Anatomical alteration of the vessels may be demonstrated in many cases." [So in the pulmonary vessels in our case.]

"Clinical experience teaches that the diminution of red corpuscles is the alteration of the blood which is most potent in determining the [acquired] hemorrhagic diathesis." [This cannot include hemophilia, where no such diminution exists.] "The hemorrhages, and the fatty degenerations of the heart, the intima of the arteries and capillaries, the hepatic cells, the renal epithelium, and the peptic glands, observed in various cases of pernicious anemia, are explained by the diminution in the mass and in the corpuscular richness of the blood." [2]

Biermer originally maintained that the visceral hemorrhages of pernicious anemia were always associated with fatty degeneration of blood-vessels. But Müller [3] failed to find this in quite a number of cases. "In these, other forms of nutritive alteration of the vessels must exist."

[1] *Am. Journ. Med. Sci.*, Oct., 1875. This opinion seems to us unwarranted, for in pernicious anemia, the diminution of red corpuscles is not accompanied by a marked increase of the lymphoid elements of the blood.

[2] Immermann, *Deutsches Archiv*, Bd. xiii., p. 217.

[3] Ueber progressive Anämie. See also Gusserow, *Archiv für Gynaek.*, Bd. ii., 1871. Ponfick, *Berlin. Klin. Wochen.*, 1876.

The fetus is tolerably well protected from the influence of the poisons capable of producing "acute fatty degeneration," in the way Buhl and Hecker suggest.[1] But, on the other hand, it is peculiarly exposed to alterations of nutrition, caused by variations in the amount of blood-supply. Disseminated lesions of the placenta, not visible to macroscopic examination, may destroy a sufficient number of villi to seriously diminish the quantity of nutritive material brought to the fetus. To an inflammation of the placenta, starting from the decidua, it is well known that Rokitansky attributed many cases of "tabescence of the fetus," and also most cases of "adherent placenta" and "placenta polypus." [2] As is also well known, the possibility of inflammation of the placenta has been denied, principally on the ground of the very small amount of connective tissue in it, and also on the absence of capillaries, except in the vascular loops of the villi.

Maier,[3] however, has more recently described a lesion accepted as placentitis by Schroeder [4] and Spiegelberg,[5] and which consists of a proliferation of the intervillous conjunctive tissue. This proliferation may begin either in rudimentary conjunctive tissue between the villi, or else in the adventitia of the arteries as a periartritis. This proliferation causes a compression of vessels and an atrophy of villi "which, if very extensive, must lead to the death of the child." Or again, the process may begin in a thickening of the decidua serotina, which becomes closely adherent to the altered placenta tissue [apparently by connective filaments binding together the free ends of the fetal villi and the maternal sinuses in which they float], so that the two can no longer be separated. From the altered serotina, innumerable processes pass into the placenta, and there connect with the increasingly thickening masses of conjunctive tissue.

Neunsam [6] apparently described an advanced stage of this same lesion, under the name, Sclerosis of the Placenta. He

[1] Hecker relates a case of "acute yellow atrophy" in a woman seven months pregnant, and ranks it with the acute fatty degeneration of the new-born. He thinks that, in both cases, some sudden decomposition of the blood occurs from causes hitherto unknown. (*Monatschr. für Geburtskunde*, 1865.)

[2] *Lehrbuch für Path. Anat.*, Bd. iii., S. 545.

[3] Virch. *Arch.*, Bd. xlv., 1869.

[4] *Lehrbuch der Geburtskunde.*

[5] *Lehrbuch der Geburtskunde*, Bd. i., 1877.

[6] *Monatschr. der Geburtskunde*, 1861, Bd. 17, p. 153.

found, in the tissue of the latter, hard white places, showing no trace of normal cavernous structure, and the hyaline basement substance of the villi changed to conjunctive tissue, granular and cloudy.[1]

Schroeder remarks that it is this "cirrhosis" of the placenta which is the common cause of its adhesion to the uterus. The chances for the life of the fetus depend upon the extent to which the lesion progresses before birth.

It is a misfortune that, in our case, as well as in the others like it which have been reported, the placenta was only cursorily examined. But the fact that it adhered closely to the uterine wall, and could only be detached with tearing, offers a strong presumption that sclerosis of the placenta existed. We may be permitted the probable hypothesis, which may easily be tested in other cases, that by such a lesion the blood-supply to the entire organism of the fetus was gradually diminished, as in the experiment of applying a ligature or a clamp to the aorta or renal artery, and that, under the influence of this oligemia, the nutrition of the blood-vessels became fatally impaired, and the glandular epithelium became fatty.

It is noteworthy that, in previous confinements, Mrs. H. had also experienced an adhesion of the placenta.

[1] Maier insists that the lesion described by him has no connection with this, but it is difficult to see why.

CONTRIBUTION TO SPHYGMOGRAPHY[1]

THE INFLUENCE OF PAIN UPON THE PULSE-TRACE

"A SUDDEN impression, however brief, made upon a sensitive nerve, always determines, as initial effect, or slackening or a diastolic arrest of the heart."[2]

In these words M. François Franck sums up the results of numerous experiments, in which the effects upon the heart, of peripheric irritations, are delicately inscribed and analysed by means of graphic apparatus. The trigeminus was irritated by vapors applied to the nose, or by rapid burning of the nostril with a red hot needle; and the laryngeal nerves, by touching the mucous membrane of the larynx with a brush dipped in ammonia: the auricular branches of the trigeminus, branches of the cervical plexus, sciatic and crural nerves, were each irritated mechanically: finally, the abdominal fibres of the sympathetic, by pinching the peritoneum inflamed by means of previous exposure to the air. "In all these cases, the arrest or slackening of the cardiac pulsations was observed as a constant phenomenon." (p. 255.) This would have passed unperceived, but for the modification introduced in the graphic tracing of the cardiac pulse movements.

It occurred to me that the foregoing experiment might be exactly reproduced on the human subject, by observing the sphygmographic tracing of the pulse at the moment that a dentist should touch the exposed nerve of a tooth. Through the kindness of Dr. Kidder, an opportunity was afforded to test this suggestion. Mahomed's sphygmograph was carefully ad-

[1] Reprinted from the *Archives of Medicine.*

[2] Travaux du laboratoire de M. Marey, année II, 1876, p. 227.

justed to the arm of a lady, who, at the time, was suffering no pain, but whose teeth were about to be filled.

Trace I. was taken while the upper part of the tooth was being scraped, an operation causing comparatively little pain;

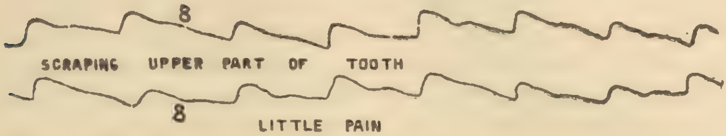


FIG. I.

the trace is regular, and the cardiac impulse strong and well sustained.

In Trace II, the upper line is taken before any manipulation of the tooth. On the lower line at B, the probing begins, and at the same moment the base line falls, to rise again, but to con-

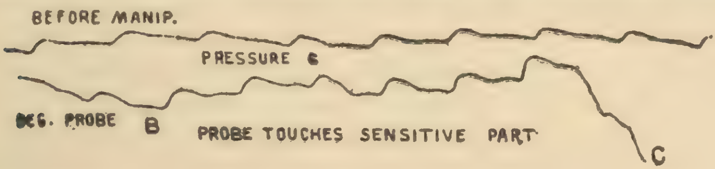


FIG. 2.

tinue somewhat irregularly. At C the probe touches the sensitive nerve, and instantly the line falls, the cardiac pulse is altogether, though momentarily, arrested.

FIG. 3.

FIG. 4.

In Traces III and IV the same fall is also clearly seen at the moment that the nerve is touched, (points B), but the fall is

not so complete, the needle not carried entirely off the paper, and the tracing is therefore resumed.

These traces, therefore, afford an interesting confirmation of the law of François Franck, that peripheric irritation of any sensitive nerve, in proportion to its intensity, inhibits the action of the cardiac ganglia. This is again a branch of the more general law established by the experiments of Goltz,[1] that the irritation of any one part of the nervous system is capable, under certain circumstances, of inhibiting the action of various other parts.

[1] *Beiträge zur Lehre von den Functionen der Nerven centren des Frosches*, p. 39.

CASE OF FACIAL AND PALATINE PARALYSIS, AND LOSS OF EQUILIBRIUM, PRODUCED BY A FALL ON THE HEAD.[1]

RALPH ROSENSTEIN, aged 2 years, was brought to the dispensary of the Mt. Sinai Hospital, on October 18, 1880; the mother stated that a week previous he had fallen off a chair to the floor, striking the back of his head. No especial effect from the fall was, however, observed during the two days first following the accident, but on the third day he began to droop; he allowed his head to fall forward, walked with a staggering gait, and finally refused altogether to walk, or even to stand. Previous to the fall, he was said to have walked well. Co-incident with the symptoms, he began to cough.

On the day of examination it was found that he could stand; could move his legs while supported in a standing position, and very freely while in bed; but would not stir from his place, even to follow the mother who pretended to lead the way. He then burst out crying, and sat down on the floor, but seemed unable to try to walk. During the examination he asked for water, and as he was drinking it was noticed that the water regurgitated through the nose. On inquiry it appeared that this had happened ever since his fall, but the mother had not thought it worth mentioning. No diphtheria existed or had existed to explain this paralysis of the soft palate. The uvula was markedly deviated towards the left. The right angle of the mouth drooped; thus there was evidently paralysis of lower branches of the right facial nerve, the staphylo palatine and buccal branches. But the upper branches were intact: the eyelids and the muscle of

[1] Reprinted from the *Independent Practioner*, Baltimore, 1881.

the forehead presented a perfectly normal appearance. There was no deviation of the tongue; none of the eyeball; the pupils were unaffected. From the tender age of the child it was impossible to ascertain whether any deafness existed on the right side or whether there was any sensation of vertigo. The morbid symptoms consisted therefore in paralysis of the right facial nerve and in loss of power to maintain equilibrium in an upright position without paralysis of the lower extremities. The difficulty of walking seemed to be entirely due to dread of falling.

Two localities suggested themselves as the seat of a lesion capable of explaining this group of symptoms. The first, some portion of the petrous bone that might include at once the facial nerve and one of the semi-circular canals of the internal ear.

It is well known at present that lesions of these canals seriously interfere with maintenance of equilibrium of the body. Experiments have even determined the direction in which each canal seems to exercise an influence. Crum Brown[1] has so analyzed these influences as to bring them within the same general law as that of the co-ordinating centres in the cerebellum. According to this law, the inclination of the body in any given direction tends to excite the centre situated on the opposite side of the body in such a manner that the complete falling over is prevented by antagonism. In the labyrinthimic canals, according to a plausible hypothesis, the nerves are stimulated when the endolymph flows in excess into the ampullæ at one extremity, the motion of the fluid being freely determined by the position of the head. The horizontal canals are all situated in the same plane, but their respective ampullæ are turned in different directions. When the head inclines to the right side, the endolymph flows from the ampullæ into the canal; while at the same time on the left side it is flowing from the canal into the ampullæ. The ampullary expansion of the left auditory nerve is therefore stimulated by the excess of pressure, and an impression conveyed to co-ordinating centres in the cerebellum, which tends to restore equilibrium. Destruction of the horizontal canal on the left side should therefore be followed by a tendency to fall to the right, from loss of the antagonistic mechanism. According to Goltz (quoted by Ferrier, "Functions of the brain,") division of the auditory nerve will, in the frog, be followed by the same symp-

[1] *Journal of Anatomy and Physiology*, May, 1874.

toms as division of the semi-circular canals. This latter fact alone could explain the circumstances of our case, supposing that the loss of equilibrium were due to injury of the petrous nerve. For although the aqueductus Fallopii carrying the facial nerve passes almost between the cochlea and the semi-circular canals above the vestibule of the inner ear, yet a lesion common to those canals and to the facial nerve, would necessitate a fracture of the bony floor of that aqueduct. The slight nature of the injury sustained by the child, and the transciency of its effects both precluded the supposition of so grave a lesion.

Again: as the lesion of the facial was certainly on the right side, injury to the semi-circular canals, if existing at all, must have been on the right side also. But, as already stated, injury of the horizontal canal on the right side, is followed by a tendency to fall on the left, on account of loss of the mechanism which naturally compensates a tendency to fall on the left. In our case the child always fell on the right.

This fact might at first seem to annul altogether the hypothesis of the petrous bone lesion. The fact that the upper facial remained intact might seem compatible with any form of peripheric lesion, and necessitate reference to the centres. The portion of the encephalon whose injury would be capable of inducing paralysis of the right facial nerve, and loss of equilibrium, is the inferior surface of the right lateral lobe of the cerebellum, near the median lobe, and also near the facial nerve after its emergence from the medulla.

"The maintenance of equilibrium is an example of adaptive, responsive or asthetiko-kinetic action, depending on the co-ordination in some central organ of certain afferent impressions with special motor adjustments. The afferent factors of this mechanism are mainly of three kinds, namely, tactile, visual, and labyrinthic impressions. We are justified in concluding that the cerebellum is the central organ of this co-ordination." (Ferrier, p. 113).

According to Nothnagel (*Topische Hiru krankheiten*), in co-ordination of movements and loss of equilibrium are the only characteristic signs of lesion of the cerebellum.[1] This is shown with special vacillation of gait, and with severe vertigo. Noth-

[1] Of course, as the author especially notices, this symptom is not exclusive to the cerebellum.

nagel adds, that if this symptom exist in cerebellar disease, it indicates a direct or indirect affection of the middle lobe of the cerebellum. According to the same authority, paresis or paralysis of the facial nerve is occasionally, although rarely observed, in lesions of the cerebellum: and is then always due to pressure upon the nerve after its emergence from the medulla, by means of some lesion, as a hemorrhage or tumor, sufficiently near the surface of the cerebellum to exercise an extra cerebellar effect. In such cases the paralysis resembles that due to lesions of the motor tract of the cerebrum in being confined to the buccal branches of the facial.

Such a paralysis would therefore not correspond to that observed in our case, where the buccal lesion was much less prominent than that of the palatine branches of the portio dura. These branches are derived from the greater petrous nerve which is given off from the facial at the geniculate ganglion." (Longet.) The geniculate ganglion is situated at the first angle of the aqueductus Fallopii, and thus lesions of the portio dura, during their passage through this aqueduct are especially liable to be attended by paralysis of the palate; a comparatively rare sequence of either completely central or completely peripheric lesion of the nerve.

The distance is very small from the geniculate ganglion to the internal meatus, where the auditory nerve still lies side by side with the facial. It is conceivable that a slight hemorrhage should occupy all this space, and thus co-incidently affect the auditory nerve, the facial, and the branches emanating from its geniculate ganglion.

The experiments of Goltz, already quoted, which, showing that section of the auditory nerve may have the same effect as lesion of the semi-circular canals, would explain why a lesion in the vicinity thus defined should occasion loss of equilibrium. The auditory nerve, though it represents neither the organ receiving labyrinthic impressions nor the central organ receiving them, unquestionably constitutes the path by which they are conveyed to that central organ, the cerebellum. A portion of the roots of the auditory nerve pass to the cerebellum in the restiform bodies, (Meyner) so that each auditory nerve is connected with the lateral hemisphere on the same side.

According to Ferrier, experimental lesions of the lateral lobes

of the cerebellum, whether destructive or irritative, are followed by the same results as are lesions of the peduncles. The displacement of the body is sometimes toward the side of the lesions, sometimes toward the opposite side. The latter, observes Ferrier, is more likely to occur when the lesion is limited, the former when it is extensive.

Hitzig, (*Untersuch über das Gehirn*, p. 203) pointed out that the passage of a galvanic current through the cranium was followed by a sudden sinking of the head towards the side on which the irritation is applied, i. e., where the anode is placed. Now an irritation transmitted to the lateral lobe of the cerebellum, along the auditory nerve, should have the same effect as this electrical irritation. Equilibrium would be disturbed from the unequal stimulus of the co-ordinating centres, and, as experiment shows, without clearly explaining why, the tendency might be to fall on the same side as the lesion. The case differs from that of lesion of the labyrinthic canals, affecting the terminal expansion of the nerve, because that implies destruction of a mechanism by which a tendency to fall towards the other side is habitually compensated, whereas, lesion of the trunk of the nerve coincides in effect with lesion of the lateral lobe of the cerebellum on the same side.

From the foregoing reasons we have ventured to diagnose a hemorrhage, extending from the internal meatus through the aqueductus Fallopii as far as the geniculate ganglion, as the cause of the symptoms observed in the case of Ralph Rosenstein.

INAUGURAL ADDRESS AT THE OPENING OF THE WOMAN'S MEDICAL COLLEGE OF THE NEW YORK INFIRMARY, OCTOBER 1, 1880.[1]

LADIES—It is a good plan, on the threshold of any important enterprise, to pause and take a survey of the field we propose to traverse; otherwise we may lose our way, and arrive at the wrong goal at last.

Every enterprise involves difficulties. Difficulties are inseparable from any condition of existence. The question therefore always is, not "Are there any difficulties to encounter?" but "For precisely what difficulties must I prepare?" The difficulties involved in the study and practice of medicine are intrinsic and extrinsic; and we will consider each in its order.

In addition it will be profitable to inquire what especial difficulties attend the study of medicine by women; and, finally, to point out some which we have practically encountered in the working of this school.

The first intrinsic difficulty in medicine consists in the great mass of facts which it is necessary to know, and in the variety of sciences which must be understood in order to interpret these facts. There is a general impression among non-medical people that all medicine can be learned simply by listening to what sick people have to say for themselves; that any one who has listened during a few months or years to such conversations knows all about medicine—is rich in experience; that what such an one does not know is not worth knowing. Now, in reality, such a method would not suffice to teach the pathology of a cold in the head, although a thousand sufferers related the details of their

[1] Reprinted from *The Chicago Medical Journal and Examiner*, 1881.

illness with the utmost loquacity. At the very outset of clinical study it is well to be impressed with this fact: namely, that what the patient has to tell you constitutes precisely the least important part of what you must learn about him in order to be able to understand his case, and to do him any good. This is not only true in regard to children, to insane people, to those who are for the time delirious or unconscious, or to those whose willful exaggerations or reticences evidently distort the description of their symptoms. It is true of every one who does not understand the pathological significance of one symptom as compared with that of another: true, therefore, of every one who is not himself a physician. Let us take an individual case—it makes scarcely any difference what. As serving to illustrate many points, I will select a case of fractured skull. The physician is summoned in haste, and learns that an hour previously the patient had fallen from a scaffolding to the street; had been picked up unconscious, and brought home in the same state; that shortly after reaching home he had vomited, but had not, as the saying is, yet come to himself. The physician finds the patient in bed, motionless and insensible. His eyes are closed, but if the lids be raised the pupils will be found to contract, perhaps sluggishly, to the light, and the lids quiver more or less if the conjunctiva is tickled. The breathing is slow and rather labored, and at each respiration the cheeks puff out as if the man were forcibly smoking a pipe. Perhaps from time to time one of the arms is raised and moved convulsively backwards and forwards, then falls again. The face is pale, but when the doctor lays his finger on the pulse he finds no sign of exhaustion; the pulse is full and hard, and rather slow. He will notice that the clothes are wet with urine. In examining the head he finds on one side, near the vertex, that the hair is matted; and, separating the mass, he comes upon some clotted blood. He presses his finger in the center of the clot, and may find a depression below the level of the cranium. Perhaps when he presses on this depressed portion the convulsive motion of the arm will re-commence. On searching farther, he may notice a clear fluid running from the ear, on the same side with the visible fracture. Here is his case. Now, for the sake of simplicity, I have so stated it that, in regard to the main fact, the doctor is not called upon to make any diagnosis. There is no doubt about it; the man has fallen and fractured his skull. But,

before the physician can understand either the extent or the consequence of this injury, he must be extremely familiar with the anatomy of the injured region. He cannot learn this anatomy from looking at the patient, nor at a hundred similar patients. He must have had the opportunity on many dead ones to dissect out all the parts, and study repeatedly their relations to each other. Then only could he know, in the first place, even that there was a brain inside the skull! Further, that the piece of bone which had been driven in by the blow had probably torn the membranes covering the brain, and even the pulpy substance of this vital organ itself. He must remember the sinuses in the membranes, and the effusion of blood that poured out from them was probably now pressing on the surface of the brain. He must be able to tell, in order to furnish the basis for his physiological analysis of the case, just what part of the surface had been injured—the part whose irritation is known to cause convulsive movements of the right arm. He must be able, from his previous knowledge, to trace downwards the direction of an invisible crack, leading from the visible fracture to the base of the skull, and splitting another portion in such a way as to allow of the escape of the clear liquid from the ear. All this knowledge, and that of other details which I omit, must the physician bring to the case from the study of the first science on which medicine reposes—the science of anatomy. He then begins to trace the relation between the symptoms he has observed and the lesion he has discovered, by means of his knowledge of the functions of the parts involved—in other words, by his knowledge of physiology. By a violent shock the functions of important organs have been rudely interrupted. The physician who was not already well acquainted with these functions would be entirely unable to explain why a blow on the head should suspend or alter them. He could not even see any reason for the suspension of consciousness, of feeling, of power of movement, which has been induced by this blow. Still less could he understand the vomiting, the involuntary emission of urine, the convulsive movements of the arm, the puffing of the cheeks, the changes in the respiration and the pulse. In other words, unless he had an intimate acquaintance with the working of the machinery of the body while in order, he would be as little able to understand its disorder as a bricklayer to know why a watch had stopped, or a

shoeblack to mend a locomotive. But the analysis of the case is not finished. The fall of a living body from a height is an event not contemplated in the physiological workings of the organism. It is effected according to physical laws, and the fracture of the skull takes place in the same way, and with the same modifications as would a fracture of any inorganic elastic globe. The radiation of the fracture, the effect of the rebound of the head from the pavement, and of the brain within the skull, cannot be studied by the aid of anatomy or of physiology alone; a third science must be invoked—that of physics. Nor is this all. I have spoken of the clear fluid running from the patient's ear. To the uninitiated this would seem to be of much less importance than the blood which matted his hair. But the physician sees in it a symptom of very serious import; he knows that it is a sign of the fracture of a certain portion of the base of the skull, and foretells almost certain death. So much he knows, or should know, as a fact of clinical experience—that is, of the clinical experience of other people; for he ought to be able to interpret this symptom as perfectly in the first case he ever saw as in the fiftieth. To understand exactly what this clear fluid is, he must, however, interrogate something else than clinical experience, for that has interpreted the matter in several different ways. The question has been solved by clinical analysis of the fluid, which has shown that it does not resemble the serum of the blood, which at one time it was supposed to be, but the so-called cerebro-spinal fluid, which bathes the brain and spinal cord, and which cannot be removed, in even small quantities, without the greatest risk to these vital organs. The gravest feature in a case of fracture of the skull is interpreted by means of the science of chemistry.

Here, then, are four separate sciences, with entirely distinct methods, with which the physician must be to a considerable extent acquainted before he can in the least understand the condition of the patient in the case we have imagined: anatomy, or the science describing the form and relative situation of organs; physiology, or the science of the functions of these organs; physics, or the science of the movements of masses; chemistry, or the science of the composition of bodies, including the solids and fluids of the animal organism. When all these have been applied to the problem, the physician is still at the outset of his

investigation. It is not enough that he sees, or even correctly understands, the condition in which the patient is; he must be able to foretell the series of changes which this condition is likely to undergo, during its progress towards death or recovery. To do this he must be acquainted with a fifth science—pathology; a science laboriously elaborated from all the experience, the observations, the clinical and post-mortem analyses which have been accumulated during the historical period of the race. Morbid anatomy is properly a branch of pathology, and nothing can be more absurd than the idea that the clinician can busy himself with the sick person during life, and leave to a specialist in "pathological anatomy" the examination of diseased organs after death. You can only properly observe the living sick man when you are able, in imagination, to pierce through the outer coverings of his body, and watch, step by step, the morbid processes which are creeping onward in the recesses of the organism. For this purpose it is essential that a science, really a branch of anatomy, but often regarded as distinct, be assiduously cultivated. I mean the science of histology. It is only when the microscopic structure of the fractured bones and torn membranes is perfectly known that the physician can understand many of the minuter morbid processes whose possibility he foresees—as an osteomyelitis, a meningitis, a capillary apoplexy. Knowing what exists, and also what is likely to occur, the physician is now prepared to intervene to help the patient, and to avert danger as far as this may be possible. In other words, having applied the arts of diagnosis and prognosis, in accordance with the laws of pathology, he is able to apply the art of therapeutics according to the indications furnished, on the one hand by surgery, on the other by the science of the properties of drugs. He will lift up the depressed fragment of bone by means of a trepan; he will apply ice to the head, to keep down hyperæmia of the meninges; he may possibly give bromide of potassium to deaden the activity of the brain when consciousness returns and delirium is imminent.

From this single illustration, you may at once learn several peculiarities of the physician's work. In the first place you have noticed that the knowledge required is not merely considerable in amount but various in kind, and that all these varieties must be co-ordinated into a single conception, which we may entitle

knowledge of the condition of the patient. In every step of the physician's career he is obliged to perform this work of co-ordination; obliged not only to know in detail, but to generalize and combine.

Now the capacity for systematic mental combination is essentially a cultured capacity, and a capacity whose effective attainment is a matter of a great deal of difficulty. It is sometimes proposed to evade this difficulty by dividing up medicine into a great number of small sections or specialties, and encouraging every one to devote himself to only one. Even were this done, the difficulty in question would not be removed, but only pushed back a little. Even when a physician professes to attend only to the diseases of a single organ, he still has to do not with one disease but with an entire class of diseases. To decide whether one of *his* diseases exists, he must know enough about a good many others to be able to exclude them from the diagnosis. Or, if he cannot do this, he must get some one else to make the diagnosis for him—that is, to take out of his hands the first large part of his own work. If we suppose these preliminary questions all decided,—and no doubt to remain that disease exists in the organ appropriated by this particular specialist—we still can only understand this by means of a mass of anatomical, physiological and clinical details, out of which he must build up the general conception of the case. Thus the mental operation is the same in kind for the specialist as for the general practitioner.

Specialists are needed for original researches, and to develop the field of medicine in such a way that it may afterward be cultivated by the general practitioner. Auscultation was once a specialty, and only a few physicians even pretended to know how to use the stethoscope. But today, as you are aware, scarcely any one claiming the name of physician would dare to disclaim his ability to do so.

It will always be desirable moreover, that certain persons endeavor to acquire unusual skill in some particular directions, that they may be called upon occasionally to decide in questions of unusual difficulty. But it must be left to the general practitioner to call in the specialist, as the judge calls an expert into court, to assist in making up the decision. The responsibility of the decision must always rest with the judge, or the physician,—after they have heard all that the experts have to say, and con-

trolled their report by means of their own knowledge of the subject, and general relations of its parts to each other.

There is another way in which a specialist may be called in: namely, like a chiropodist to attend to some entirely subordinate and presumably insignificant detail. Whoever adopts a specialty for the sake of narrowing his knowledge, and not in order to deepen it, is liable to become a specialist of this kind—a mere corn doctor; with no valid claim to membership in a liberal profession.

We return therefore to our assertion that it is impossible for a real physician to escape the necessity of constantly dealing with multiple groups of facts. He cannot therefore be dispensed from the necessity of acquiring the mental culture which alone can enable him to accomplish this task. To further illustrate my meaning, I would point out that there are four successive degrees of generalization that may or must be effected by the physician. The first degree is that which I have already shown to be involved in the very simplest diagnosis of disease in a single organ of the body. In a second degree of complexity the physician is obliged to consider also the co-existence of a morbid condition in some organ, and to ascertain which, if any, are the relations between these two. Thus, if the same patient be suffering from dyspepsia and endometritis, it is very important to know whether the dyspeptic symptoms result from the irritation of the endometritis, or whether the endometritis is the final expression of a state of denutrition originated by the dyspepsia. If, again, a pregnancy complicates the uterine disease, the question of treatment is rendered more difficult by the risks of interfering with the pregnancy.

In a third degree of generalization, the physician must rise to considerations of the pathogeny of disease, and these are inseparable from general philosophic notions to enable him to grasp the theory of the matter. Thus, in investigating a case of phthisis, the physician will go but a little way who rests with the report of subcrepitant râles at the apex of one of the lungs. It is imperative that he understand the theory of phthisis, and the relations between the theory of Bayle and Laennec, which would attribute these râles to ruptured tubercle; the theory of Rindfleisch, which would explain them by the breaking down of masses of tissue chronically inflamed; the theory of Buhl. explaining the ulceration process by a diphtheritic-like infiltration.

Immediately or remotely, the practical treatment of phthisis is moulded by the theory which may have been adopted.

Similarly, the practical treatment of uterine diseases must vary considerably when the theory of menstruation regards this process as a congestion, or as a plastic process of growth.

The highest degree of generalization is that involved in the pursuit of original researches. Upon this we will not now stop to speak.

Now, as I have already said, the capacity for generalizing is essentially a cultured, an acquired capacity. Whenever it seems to be natural, that is, to come without any special training, it is always wrong. That is to say, untrained persons of active minds, and who are often very ready to generalize, invariably do so from too small a number of facts or data. Hence their conclusions are inadequate or absurd. Homœopathy furnishes an excellent illustration of just this kind of generalization. It has picked up a superficial resemblance between things; has refused to analyze further the real relations of these things, and then insists upon having discovered the true theory of their relations. Thus Hahnemann gives as an illustration of the way in which natural instinct appeals to the law of Similia: the case of a cook who, having burned her finger, plunges it into warm water; or, the boy whose fingers are frost-bitten, yet who takes care to rub them with snow. Now this accidental resemblance between the cause of the injury and the treatment explains nothing. A little deeper examination shows that, in the first case, the warm water is required to relax the distended blood vessels; in the second, the cold is needed to restore the circulation gradually and not with a rush, which might prove fatal to the tissues. In these celebrated examples, an immense fallacy is accepted, by omission of the philosophical distinction between two kinds of causes: the efficient cause, the burn, which has initiated a train of morbid processes; and the proximate cause, that is, the anatomical and physiological conditions upon which the symptoms immediately depend.

To train the mind to handle large masses of facts, it must be gradually accustomed to work with somewhat smaller masses of more accessible facts. This is the reason for that general literary education which, in all European schools, is exacted as an indispensable preliminary to medical study, and which,

in this country, is often considered as superfluous. But it can only be so considered by those who have never tried to analyze the mental operations involved in the simplest medical work.

Our illustrative case shows that something else is necessary also. The senses must be trained as well as the mind. I will not now dwell upon the methods for training the senses, but only point out two facts. First, that the facility and accuracy with which the senses work, is largely in proportion to the amount of mental training that guides their operation. You can see, hear and feel a hundred fold more when you know before hand exactly what is to be felt, or heard, or seen; and when you have an ideal standard with which you can compare the results furnished by your eye, ear, or finger. In the second place, it is logical and much easier to train the senses by means of simple exercises before attempting more complex ones. Thus an excellent preparation for learning how to observe in anatomy, is to pursue observations in botany.

It is now worth while to inquire, since the study of medicine is so vast, what proportion of it can possibly be mastered during a given term of years: in other words, what we may expect a student to know who presents himself for graduation. As the foundation of everything, a really complete knowledge of anatomy is indispensable. It will not do to know that an artery is, as the boy said of Abraham, "there or thereabouts." It will not do to have a general idea that the nerve centers are divisible into a cerebrum or cerebellum, medulla and spinal cord. The anatomical knowledge that is not precise and accurate is as unavailable for the physician as would be a general idea of the county in which a person lived, to the postman charged to deliver a letter to him.

There is another reason for demanding completeness of knowledge in regard to coarse and fine anatomy, and that is, that it is so readily forgotten in after practical life, and requires to be so constantly revived by fresh reference as wanted. Students are apt to think that therefore it never need be fully known at any time. This is a great mistake. What has once been firmly stamped upon the mind, can easily be revived; what has always been vague, will always remain so, unless there take place such a radical change in mental habits and methods as we have no great

reason to expect. The science of chemistry, so far as regards its relations to medicine, should also be perfectly known at the outset, and can be known because these medical relations of chemistry are at present comparatively so few. Physiology, on the other hand, embraces a much wider field—more indefinite and more complex details. The knowledge acquired of it during a medical curriculum, must be small as compared with the relative amount attainable in anatomy and medical chemistry. But absolutely, this amount is considerable. It is of the greatest importance that the student learn to distinguish the different degrees of certainty which exist between the various physiological doctrines he hears enunciated. It is in studying them that he is first introduced to the peculiar difficulties of the study of medicine, inherent in its imperfection, in its complexity, and in its progressive character. It is impossible to study physiology by the memory alone. Even to remember its details requires a habit of mental poise—a capacity for criticism and judgment which is only acquired by very careful training. In testing the candidate therefore, we expect to find, not a complete knowledge of physiology, but an *accurate* knowledge of certain fundamental facts, familiarity with accepted methods for both the acquisition and application of physiological knowledge, and some trained judgment in regard to the grouping of facts known; finally, sound and vivid perceptions of the relations of physiology to medicine, and of their constant interdependence upon one another.

Coming now to medicine proper—what may we expect a graduating student to know? It is a mass of knowledge so vast (often *so confused*), so unsystematically grouped together—so largely empirical,—so unequal in its development; its acquisition depends so much upon prolonged clinical experience with personal responsibility, that it is really very difficult to define just how much may be acquired; how much and what must be expected of any one after a given course of study. We can, however, say this: First—That the graduate must be thoroughly acquainted with the rules of diagnosis, and show his ability to apply them in any given case. Second—That he must be acquainted with the typical outline of all classical diseases, and thus know the symptoms upon which the diagnosis is based. Then there will be nothing to prevent him from diagnosticating even the very first case he ever sees of even the rarest disease. Whoever is able to

do this; whoever has reached a standpoint from which he can scan the entire horizon of medicine, has reached a beginning whence nothing need prevent indefinite progress. But unless this beginning be reached, the physician is really incapable of making the first decision about any one who comes to his office, or who calls him to their bedside. A doctor who did not know that coryza was one of the symptoms of syphilis, could not safely pronounce with positiveness upon the nature of an apparent cold in the head. Another, who knew of no eruptive fever but measles, would certainly be incompetent to decide that the rash in a given case were not scarlatina or small-pox. No young physician can be expected to know all about all diseases; but he must be acquainted with at least the existence of all that there is to know about. And he must, moreover, have attained sufficient mental breadth and grasp to be able to keep the recollection of all firmly and clearly before his mind at the same time. Now this knowledge is really quite attainable by a curriculum of three or four years' duration, if the study be sytematically and intelligibly pursued.

The art of therapeutics is much more difficult of acquisition. The treatment of a disease involves many more considerations than even does its diagnosis; and these are susceptible of much greater variety in grouping. Surgical therapeutics, or, as you would perhaps call it, operative surgery, is much the simplest, and, accordingly, is much farther advanced. The logical method would prescribe that before studying the effect of drugs internally administered, the pupil should be carefully trained to watch the effect of the topical applications, the various manœuvers and operations, by which a surgeon deals with cases of external pathology. The question that meets us at the outset is, Is it really possible for us to produce any definite effect upon the processes of a living organism? This question is at present better answered in surgery than in the domain of internal medicine; and we should therefore seek for the answer first there. Yet so easily are we deluded into believing that whatever is familiar is simple, and whatever is unfamiliar is abstruse, that I suppose there is not one of you who would not believe that the action of a dose of castor oil was much easier to understand than the action of a fracture splint; or, again, that any woman physician might be expected, in virtue of her sex, to know something of pessaries,

but need not be expected to know anything of orthopœdics, although a pessary for the replacement of a dislocated uterus is strictly a surgical and, by analogy, at least, an orthopœdic apparatus. What we may expect of a student at graduation is, to know the precise physiological action of drugs so far as this is known at present; to know the principal variations in such action occasioned by disease; to know the principal indications for the use of the drugs; and, finally, the principal diseases in the course of which these indications present themselves. It is unnecessary to add that he must know the doses and preparations of these same medicines.

To apply my previous test, I would say that the theoretical possession of this amount of knowledge is quite attainable in three or four years. The practical availability of it, is attainable with such slowness and difficulty, that it would really be desirable to pass a law forbidding any young physician from assuming the full responsibility of prescribing until, for a year, privately or in hospitals, he had practiced under the close supervision of some one else.

The work of co-ordinating multiple facts, which I have said was the characteristic work of the physician, must be begun by the student in his most elementary attempts at mastering knowledge. This is the only way in which he can remember the immense amount of facts he is expected to know. He must bind them firmly into a single bundle, or a definite number of single bundles, or they will all fall apart like scattered sticks.

Every time you learn anything new, you should stop and ask yourselves whether you know everything which is implied in that knowledge. In studying the anatomy of muscles, you have an opportunity of reviving your knowledge of the bones on which they are inserted. In studying the course of arteries and the distribution of nerves you refresh your recollection of the muscles which serve as landmarks to them. In observing any case of disease in the college clinics, it should be your self-imposed duty to ask yourselves if you know all about the anatomy, histology, and physiology of the organs involved in the disease. The constant, faithful, patient repetition of these inquiries would continually render the co-ordination of your various studies more and more easy to you; would train you in the capacity, invaluable in a physician, of bringing to bear all your knowledge at any given

do this; whoever has reached a standpoint from which he can scan the entire horizon of medicine, has reached a beginning whence nothing need prevent indefinite progress. But unless this beginning be reached, the physician is really incapable of making the first decision about any one who comes to his office, or who calls him to their bedside. A doctor who did not know that coryza was one of the symptoms of syphilis, could not safely pronounce with positiveness upon the nature of an apparent cold in the head. Another, who knew of no eruptive fever but measles, would certainly be incompetent to decide that the rash in a given case were not scarlatina or small-pox. No young physician can be expected to know all about all diseases; but he must be acquainted with at least the existence of all that there is to know about. And he must, moreover, have attained sufficient mental breadth and grasp to be able to keep the recollection of all firmly and clearly before his mind at the same time. Now this knowledge is really quite attainable by a curriculum of three or four years' duration, if the study be sytematically and intelligibly pursued.

The art of therapeutics is much more difficult of acquisition. The treatment of a disease involves many more considerations than even does its diagnosis; and these are susceptible of much greater variety in grouping. Surgical therapeutics, or, as you would perhaps call it, operative surgery, is much the simplest, and, accordingly, is much farther advanced. The logical method would prescribe that before studying the effect of drugs internally administered, the pupil should be carefully trained to watch the effect of the topical applications, the various manœuvers and operations, by which a surgeon deals with cases of external pathology. The question that meets us at the outset is, Is it really possible for us to produce any definite effect upon the processes of a living organism? This question is at present better answered in surgery than in the domain of internal medicine; and we should therefore seek for the answer first there. Yet so easily are we deluded into believing that whatever is familiar is simple, and whatever is unfamiliar is abstruse, that I suppose there is not one of you who would not believe that the action of a dose of castor oil was much easier to understand than the action of a fracture splint; or, again, that any woman physician might be expected, in virtue of her sex, to know something of pessaries,

but need not be expected to know anything of orthopœdics, although a pessary for the replacement of a dislocated uterus is strictly a surgical and, by analogy, at least, an orthopœdic apparatus. What we may expect of a student at graduation is, to know the precise physiological action of drugs so far as this is known at present; to know the principal variations in such action occasioned by disease; to know the principal indications for the use of the drugs; and, finally, the principal diseases in the course of which these indications present themselves. It is unnecessary to add that he must know the doses and preparations of these same medicines.

To apply my previous test, I would say that the theoretical possession of this amount of knowledge is quite attainable in three or four years. The practical availability of it, is attainable with such slowness and difficulty, that it would really be desirable to pass a law forbidding any young physician from assuming the full responsibility of prescribing until, for a year, privately or in hospitals, he had practiced under the close supervision of some one else.

The work of co-ordinating multiple facts, which I have said was the characteristic work of the physician, must be begun by the student in his most elementary attempts at mastering knowledge. This is the only way in which he can remember the immense amount of facts he is expected to know. He must bind them firmly into a single bundle, or a definite number of single bundles, or they will all fall apart like scattered sticks.

Every time you learn anything new, you should stop and ask yourselves whether you know everything which is implied in that knowledge. In studying the anatomy of muscles, you have an opportunity of reviving your knowledge of the bones on which they are inserted. In studying the course of arteries and the distribution of nerves you refresh your recollection of the muscles which serve as landmarks to them. In observing any case of disease in the college clinics, it should be your self-imposed duty to ask yourselves if you know all about the anatomy, histology, and physiology of the organs involved in the disease. The constant, faithful, patient repetition of these inquiries would continually render the co-ordination of your various studies more and more easy to you; would train you in the capacity, invaluable in a physician, of bringing to bear all your knowledge at any given

point, and of turning it to account wherever it was wanted. For here is the immense peculiarity of medical knowledge—it must all be turned to account. It is tremendously, often terrifically, responsible. It is this sense of responsibility which should be constantly impelling the medical student to a determination to *grasp* a subject, instead of remaining content to wabble about in it. Medical knowledge is not something which can be purchased and applied to a patient like a plaster or a poultice; it is something to be handled—like a tool, like an ax—and the effectiveness of the handling depends upon the firmness of grip of him who holds the instrument. This fact involves a double responsibility on the part of the teacher. It is not sufficient to expound doctrines and convey information; it is essential to train the minds of the persons who are expected to profit by it. This requires systematic intellectual gymnastics; requires repeated practice in all the mental operations which, in after life, the student will ever be called upon to perform. Thus he must be taught, he must probably be compelled, not only to remember approximately, but accurately; not only to be able to think when at leisure and unencumbered, but under strong pressure, and perhaps in the midst of the most embarrassing circumstances; to express himself, not only in a slovenly, awkward, halting manner, calculated to make nervous people impatient, and timid people alarmed, and arrogant people contemptuous, but in such a clear, concise, forcible way as shall always compel attention and extort respect from the very midst of hostile criticism. The physician, like the soldier, must be trained to act under fire; and a training for mere holiday manœuvers, out of sight of the enemy, is lamentably insufficient for the purpose. Human minds are not pint-pots, into which we may pour water or milk or wine at our option; nor are they often Danaides, which may be quickened simply by immersion in a golden rain from heaven. They are living organisms which can only use what they have assimilated and digested, and wrought into the texture of their inmost fibers.

This vigorous assimilation demands qualities of grit, which are as much moral as intellectual. Many moral qualities are needed in the practice of medicine to meet the difficulties which, though extrinsic to the case considered as an intellectual problem, are very important in its practical discussion. The fundamental

difficulty of all lies in the fact that so much depends not only on rigid adherence to rules (and there are many more rules for guidance than you might sometimes suppose), but that nevertheless the final arrangement must be left to the individual tact, discretion, and judgment of the practitioner. The theoretical and practical are inextricably intertwined; and the promptness with which theories will often be found to effect modifications of practice, in itself renders medicine one of the most interesting spheres of human existence. Hence in the most abstract reasoning—if the physician be capable of such—he must always keep his mind intently focussed upon the practical purpose towards which it must converge. He must see all his reasons, not hovering about in the air, like bodiless cherubs, around the bed of his patient; but embodied in tangible facts and definite actions. He must see that his antiseptic fluids actually reach the infected surfaces; he must see that his hot baths are of a given temperature, and that his cold applications are renewed as often as they grow warm; he must know whether the medicine prescribed has been vomited, whether the food has been given at the stated intervals, whether the pulse has responded to the stimulant. He must know how to enforce his directions, in spite of the reluctance, or indifference, or carelessness, or stupidity, or forgetfulness of his patients; in spite, moreover, of the interference of friends, who invariably try to persuade the sick person to call in another doctor. In many cases the physician must almost, as it were, carry his patient in his arms, encouraging, urging, consoling, inspiring him. To do this he must be capable of sympathy with physical suffering, at once delicate and profound. To be efficacious, this sympathy must be fine, and not blubbering; it must feel for the patient ten times, a hundred times as much as it audibly expresses for him; it must manifest itself in deeds, not in words; in indefatigable efforts to accomplish the essential, not in rambling talk about irrelevant trifles, even when, to the sick person, these seem to be the most important.

And at the same time, while treating his patient as though he were a personal friend—while, if necessary, risking his life for him—the physician must never forget that this same patient is, from the nature of things, a possible enemy. A physician prescribes somewhat as the Spartans under Lycurgus were permitted to propose a new law. If the proposition succeeded, the innova-

tor was honored immensely; but if it failed he was put to death. By the most scrupulous honor and the most conscientious care, the physician is bound to justify a claim to the absolute confidence of his patients; but he must never give them his. He must never be off his guard; never forget that he is the object of incessant criticism, not only for what he does, but also for what he does not do, and for every detail of his way of doing. It is essential that in every detail, in every expression, in the entire mental atmosphere of the physician, the patient should feel himself in the presence of a superior person. He must be conscious that a mind warm, vivid, and penetrating is dealing with his case. He must be conscious, also, that notwithstanding this personal sympathy, the physician is studying his case as coolly, impartially, abstractly, as if it were a problem in algebra. If he does not do so—if, moreover, he fail to solve the problem—sooner or later the patient will leave him, perhaps with the best good wishes, but still he will leave him, and try his fortune elsewhere.

You see, therefore, that, in order to be a physician, it is not sufficient to have a good memory and be able to pass examinations. This is indispensable, but much more is required. The capacity to examine minutely, yet generalize comprehensively; to take large views, yet not overlook the smallest details; to be quick to notice, yet slow to speak; to reason cautiously, yet decide promptly; to be at once very cool and very warm; to be tenacious of one's reputation, yet indifferent to careless opinions; to be sensitive, yet not touchy; to be patient in temper, yet capable of wrath; to be absolutely honest, yet successfully prudent; to be unworldly, yet capable of managing the forces of the world—all these mental and moral capacities are necessary to enable a physician to study practical medicine, to practice medicine, and to build up a practice out of services rendered to a crowd of sufferers, at once helpless, ignorant, exacting, and capricious. Varied as are the mental and moral capacities required for this enterprise, they may be all traced back to three, namely: Ability to think, character to control, and honor to act from an internal instead of an external standard of obligation. When these qualities are not possessed, or have been insufficiently developed, one of two things happens. Either, in the competitive struggle, the ill-prepared physician gets crowded out by more capable rivals; or else, he manages to hold his place, but at the expense

of patients, ill-treated by him, and who might have been better treated by some one else.

These patients are the persons who must be kept in view by the examining boards, who are licensed with the power to grant medical diplomas. This power constitutes a tremendous social responsibility. It is quite possible for a medical college to have no other function than that of testing candidates. This is the case with the University of London. It gives no instruction at all, but it grants degrees to all persons, who, having been educated elsewhere, are able to pass the scrutiny of its examiners. It must always be the principal function of a medical college to fix the standard of attainment;—and to point out what must be learned and what must be done to reach this: and thus, finally, ascertain as far as possible whether candidates have fulfilled these conditions. The college is then able to turn to society and say to people entirely helpless to judge for themselves: "Here is a person to whom, in perfect confidence, you may entrust your most important interests. Upon his knowledge, his skill and judgment you may rely as completely as upon that of any one of equal professional age in the profession; and upon his honor, you may at once rely absolutely." The responsibility attaching to this assertion is so tremendous: the consequences of a false assurance of confidence may be so various and so disastrous, that in comparison with it, sympathy for the disappointment of an unprepared candidate ought to be left entirely out of sight. The examining board betrays its social trust the first moment that it consents to confer a certificate of capacity upon an incapable person. In such a case, it becomes culpable of the same crime, for which, after the recent Seewanhaka disaster, the grand jury indicted the inspectors to whose false assurances of security that terrible disaster was traced.

This consideration comes up with especial force in regard to women medical students. These are still, by the majority of the public, regarded as disqualified from the practice of medicine merely by reason of their sex. The same reason is not always given. It is sometimes alleged that they have too little mental capacity; sometimes too little general education; sometimes too little physical health; sometimes that their judgment is too flighty; sometimes that their temperament is too excitable; sometimes that they have too little self-reliance; sometimes that they

have too much self-assurance. But that, whatever be the reason, they are intrinsically unable to make, or to be made into, safe practitioners.

When you have assembled together in an institution legally chartered and recognized by the State for instruction in medicine; when you find yourselves going through the same exercises as those which are being carried on in every other college in the city: ultimately brought to a commencement hall, where a band of music and a valedictory address seem to imitate to perfection those of the best equipped universities, it is not unnatural for you to feel as if all this vexed question about women's capacity for the profession of medicine had been entirely settled. In reality, however, it is not so. It has almost reached the point where it can be decided on its real merits, and on the actual results of the work done by women as physicians. But it has not quite reached even this point, since the preparation afforded to the mass of women students is still inferior to that which is attainable, if not attained by men. In the meantime, although skepticism has become more polite, or veiled, it is still much more wide spread than you would probably imagine. Only a few months ago a prominent physician of this city expressed the doubt,—in private conversation it is true,—whether, in twenty-five years from now, any women would be found practicing medicine. A professor of Ann Arbor has recently written two letters to a Michigan paper to express himself as "decidedly adverse" to the attempt of women to practice medicine. A few years ago, one of the lady trustees of this college told me that a friend of hers asked her why she had anything to do with women doctors, when it was notorious that they were all Free Lovers. Last year another lady trustee explained the indifference of so many influential people to the success of this school, as compared with their interest in the Training School for Nurses, on the ground that the latter were felt to be a necessity, while a medical school for women could only add a poorer class of doctors to an already over-crowded profession. There were more doctors turned out now every year than could find work to do in the community; there was not really any reason for helping to manufacture more. When I suggested that some of the women doctors were expected to displace a certain number of men, she was perfectly astonished. She tacitly took for granted that all the men must first find some-

thing to do: what was left over only, could be taken up by the women.

But now this is the very point at issue. Since society is, numerically speaking, already supplied with quite enough doctors, the only way in which women physicians can possibly gain any footing is by displacing a certain number of men. In order to do so, they must evidently show qualifications superior to those of the physicians whom they displace, and sensibly equal to those of the physicians with whom they are to be ranked on an equality.

Now, it is well to at once recognize the fact that a good many difficulties stand in the way of both achievements, and these can only be surmounted when they have been distinctly recognized and systematically provided for.

It is very difficult for women to make headway against the settled opinion of society that they are unfit for final responsibilities. This opinion not only often hinders their education to responsibilities, by preventing people from entrusting them into their hands; but it reacts upon their own minds, is liable to make them hesitating, undecided, timid, and thus still more to justify the social prejudice. It is a common remark, "Women do not feel any confidence in women; in an emergency, they must always appeal to men." This, because it is the habit of centuries so to appeal; because the mass of knowledge, power, and force is still overwhelmingly on the masculine side; because, perhaps, the mass of such force always will be so distributed, and the women in positions of first-class responsibility will always be sufficiently in the minority to be deprived of the benefit of traditional influence and prestige. The claim to equal confidence as made by a woman must be a peculiarly intellectual one, because it must be sustained in spite of a conspicuous inferiority of physical strength. To produce upon the mind of the average public the same impression as may be made by a masculine physician, the woman must exhibit comparatively more force of mind and character, because the force of body is so much less, and in a question of forces the impression unconsciously received from physical size must be taken largely into account. It is like a watch as compared with a locomotive; if there be not greater precision of action in the one, to balance the imposing massiveness of the other, the more delicate instrument must be crushed with con-

tempt. Many mental habits of women stand in the way of their acquiring this superior precision and surety. These can only be acquired by means of repeated tests, and by the prompt rejection of all work which does not come up to a given standard. But women, as a class, are never habituated to test their work; and have an almost irresistible tendency to appeal to some personal influence to avert the consequences of its failure. I do not wish to make any protest against the habit of appeal to personal influence; it is ingrained in the nature of things and of women, and when restrained within its proper sphere does a great deal of good. But it certainly has a tendency to deteriorate the character of women's work, unless they strenuously resist it.

In the general theory of society, women are not expected to achieve anything. This theory is sometimes the reason that they are not trained to achieve anything—that their education is so flimsy and scrappy; sometimes, again, on account of this theory, so much surprise is elicited when they do achieve ever so little, that they are flattered into a very dangerous over-estimate of their own powers. In this flattery there is often concealed the feeling expressed by Dr. Johnson in his celebrated remark about a woman preaching: "It is," he said, "like a dog standing on its hind legs—it is not well done; but then the wonder is that it is done at all." The tendency of women to nestle within a little circle of personal friends, and to accept their dictum as the ultimate law of things, renders them as liable to be spoiled by this sort of admiration, as they are liable to be discouraged when they do not get any admiration at all.

The remedy for all this, however, is not hard to find. A woman must accustom herself to dispense with the personal approbation of the people she knows, as a stimulus for exertion. She must learn to work for the sake of the work; she must be ready to put into it an amount of labor as would not "pay" if estimated merely by what can be seen on the surface; she must know how to hold her own standard a good deal higher than that of partial friends; she must learn, not only to keep calm under blame, but, what is much more difficult for a woman, to bear praise unmoved, otherwise she will soon cheapen with the praise. The careful self-education of women in all these matters is so much the more important, because it is only by means of it that they can hope to overcome the more external difficulties by which

they are weighted. It will not do to forget that their health is often fragile; that they often begin to study somewhat late in life, and when much needful vitality has been exhausted; that they are more frequently involved in family responsibilities and complications. At any rate there is always one two-fold dilemma. They are either pecuniarily well off, and then the force of tradition tends to keep them from working, because, as it is said, there is no occasion for it; or else they work—they study medicine, for instance—under such pressure of pecuniary necessity as leaves them barely the time or the means for adequate preparation. It is comparatively rare that the happy mean exists, where the student possesses just enough money to secure her from want, yet not enough to take away the stimulus for exertion. This is exactly the amount required.

The question of marriage again, which complicates everything else in the life of women, cannot fail to complicate their professional life. It does so, whether the marriage exist or does not exist, that is, as much for unmarried as for married women. In my opinion the increased vigor and vitality accruing to healthy women from the bearing and possession of children, a good deal more than compensates for the difficulties involved in caring for them, when professional duties replace the more usual ones, of sewing, cooking, etc. But in this delicate and important matter the facility of adjustment will vary in every individual case. Many married women will lose all interest in medicine as soon as they have children, as many now fail to develop the full needed interest precisely because they have no other, and are dispirited by isolation from family ties. Many will interrupt their practice during the first few years after marriage to resume it later. Whatever is done, either with or without marriage, can evidently be well done only in proportion as more complete intellectual development and more perfect training enables the woman to cope with the peculiar difficulties inherent in her destiny.

Women may be said to have obtained a foothold in medicine in modern times on account of the sudden development of gynæcology. It cannot be said that women have contributed much towards this development; but in the treatment of uterine diseases the desirability of women physicians from motives of delicacy, becomes so evident, that a powerful impulse has been created in favor of allowing them to practice at least this branch

of medicine. From what I can learn, the majority of women who study medicine do so with the expectation of at once becoming specialists: and certainly, the majority of persons who think of consulting them, think of them first and foremost, if not exclusively, in this connection.

Now, nothing can be more certain than, if women are enabled to practice medicine only in this specialty and for this reason of delicacy, they must, sooner or later, be again excluded from medicine altogether. I say again, because as you know or should know, women have at many different times been admitted to the privileges of medical studies and practice, but have never gained so firm a footing that they were not liable to be displaced. The motive of delicacy; the motive of self-support; the motive of desire for wider spheres of action, are all perfectly legitimate motives, but they are extrinsic to the real reason for the existence of any class of practitioners. This reason is, that such a class is in possession of knowledge which enables it to understand disease, and to cure the sick, and which justifies its members in assuming full responsibility. This full responsibility cannot be assumed, except after liberal study of the whole field of medicine. If, at present, here and there a specialist may arrive at distinction who really only knows one thing: he can only do so because the mass of the profession know a great deal more. If an entire natural class of people devoted themselves exclusively to one thing, they would soon not know even that. Instead of obtaining a position superior to that of the rest of the profession, they must sooner or later sink to an inferior one. In the case of gynæcology and women, the practical experiment has been made: the services of women have been sought on a large scale exclusively from motives of delicacy, and you know in what way. The women were merely assistants—employed to make uterine examinations and report to physicians who were strictly forbidden to make such examinations themselves. The women experts learned as little of the subject as Milton's daughters did of the Latin they read to him without understanding it. The progress of science was retarded, and their intervention was finally discarded as cumbersome. If women will use this specialty, now often thrust upon them, as a stepping-stone to general medicine; if they will look upon it as the small end of a wedge, and persist in driving it forward to a larger end; then they may assure their

position, and that of their successors, by means of this temporary opportunity. But if they do not obtain a foothold on the broad, intellectual basis of general medicine; if they content themselves with claiming this little corner, they will never really gain a high place even there: they will be driven out, little by little, until at last the gynæcological wave may pass by, and leave them stranded. There may be less liability to uterine diseases; or these may be so much more easily foreseen and prevented that much less "local treatment" remains to be instituted; or the sentiments of delicacy may change. Just imagine what would become of a class of physicians now-a-days who had devoted themselves exclusively to the treatment of scurvy or of leprosy! Their occupation would be gone with the disappearance of the disease; and the boon to humanity would result in ruin to their class.

I wish now, in concluding, to call your attention to a last class of difficulties, especially connected with medical schools for women. These difficulties all arise out of one fact, namely, that there are not as yet a large enough number of women studying medicine to support medical schools on a large scale; and schools on a small scale are inadequate, because there is no such thing as large or small in medicine.

During the thirty years which have elapsed since women first began to study medicine in America, there have always come forward a much larger number to claim the right to practice than to crave the privilege of being thoroughly well educated. This unfortunate majority has been the cause of immense injustice to the higher toned minority, because they have constantly tended to drag the conditions of medical education down to the level of their capacity, or intention to fulfill them. The competent have often been sacrificed, in order that the incompetent might be satisfied. A Nemesis never fails to wait upon inefficient intellectual work. It invariably grows lifeless, dull, uninteresting; it finally ceases from sheer inanition. On the contrary, nothing more is required to quicken any subject or any occupation into the most vigorous life and fertile interest, than that every one engaged in it should be inspired with an ardent desire for knowledge and for high attainment. Whenever people are content to do a thing in a slovenly and wanton manner, they very soon get to the end of it. But whenever they try to do it as well as it is

possible to be done, or try to learn everything about it that any one else knows, they find themselves at the beginning of a task to which there is no end. They find more to do every day; every day, also, they find more power to do it.

If all the students of this, or any other school, were thoroughly imbued with the determination to accomplish the work before them in the best possible manner, many of the difficulties inherent in the comparative smallness of the school would vanish. You should learn to look at yourselves as a colony just landed in a new country; compelled to found a state in spite of hardship, and peril, and danger, and isolation, by means of the vigorous and intelligent co-operation of each of its members. I do not know that any more instructive reading can be found than the history of colonies, a theme with which every American certainly should be thoroughly familiar. In studying the various destinies of the early settlements of this country, you may gather many hints of importance applicable to our present situation. For us, also, the sea has been traversed, the landing effected, the howling savages, represented by the medical students, temporarily repelled. But that is about all which has as yet been done. It remains to be seen whether our colony contains in itself the stuff out of which the Bay State was built up; or rather those vicious and corrupting elements which corroded to destruction so many settlements south of the Potomac. And do you know what was the one predominating influence that led to such destruction? It was that the mass of gentlemanly emigrants, who had not learned how to dig, and who were by no means ashamed to beg; who had left the mother country, not to seek an opportunity to work more, but to work less; to shirk all the work they possibly could; to profit by the industry and courageous patience of their companions, in order to share, without due share of labor, the revenue accruing from their tobacco and their corn. These are not the characters which could have founded Massachusetts and laid the corner-stone of that State, where, a century later "embattled farmers could fire the shot that echoed round the world."

Theirs is the stuff, these are the characters, this is the austere, self-denying, intelligent heroism, which is needed for our enterprise—for this also still deserves to be called heroic.

SPECIALISM IN MEDICINE [1]

We propose to consider briefly, but critically, the following proposition, which, though not distinctly formulated, is, as it were, held in solution in many others now current, and may be easily precipitated from them.

At the present day medical science has expanded to such an extent that its intelligent cultivation as a whole by any one person has become impossible. The practice of medicine, therefore, to the extent to which it may reach any really high standard of excellence, must henceforth be carried on exclusively by specialists.[2]

Thus, the physician, who should, in chimerical imitation of Lord Bacon, propose to "take all (medical) knowledge for his portion," must, on this theory, be consigned to a limbo of wornout inanities. Nevertheless, the most useful functions of specialists are still exercised with tacit reference to the intelligent practitioner, who is compelled, not indeed to know all about all medicine, but to hold the key of admission to any of its branches, of which, at any moment, he may have practical need.

Thus, specialists are justly expected to become the depositories of special literature, and to so sift, handle, classify, and arrange this, that it become accessible to, and utilizable by the general practitioner. By reiterated experience, they are expected

[1] Reprinted from the *Archives of Medicine*, 1882, vol. vii.,—Editorial Department.

[2] . . . "The fact, the hard and undeniable fact, that all intelligent and scientific physicians are quasi-specialists, and must be. In the present development of medical science there is no alternative; a physician must be a quasi-specialist, or possess a universal knowledge of a superficial, mostly booky kind, —a knowledge wholly insufficient to insure intelligent or successful practice." E. C. Seguin, these ARCHIVES, April, 1881, p. 186.

to acquire an exceptional familiarity with certain types of disease, so as to be better able to decide in rare, obscure, or unusually difficult cases, when the physician shall call them in. By continued application they may tend to indefinite improvement in the technique of diagnosis and of treatment. Finally, in regard to the state of medical knowledge on any given question at a given moment, they may furnish the standards with which the knowledge and practice of the general physician must constantly be compared and tested. Thus, specialism is largely useful in furnishing the *exact* material with which the general physician may make his practical combinations. In his absence, and from the languid interest which specialists profess in each other's departments, this combination would often not be effected. But the problem offered by a sick person is always a problem of combination. The practical specialist does not analyze, but roughly divides this problem according to considerations frequently artificial. The scientific specialist abstracts phenomena completely; studies separately, anatomical, physiological, chemical, pathological conditions. It is the ideal business of the physician to take conditions which science has abstracted for the purpose of thought, and to recombine them for the purposes of life. In the absence of the physician there would be no one to do this; with every new deterioration of the ideal character of the general physician, this work of combination is less and less well done. As a consequence, every sick person who can pay for it begins to expect to divide up his body among a cluster of "eminent specialists" before any positive diagnosis of his case can be reached.

Notwithstanding the inconvenience and expense of this procedure, it tends to gain in popularity on account of the simplicity and apparent common-sense of its theory. The laity are very ready to infer not only that specialism is good, but that the more of it the better. If the physician who treats six diseases is necessarily superior to him who is willing to manage sixty, then he who confines himself to one must be the best of all. Hence the popularity of the pile doctor, and the cancer doctor, *et hoc genus omne.*

The great principle of unity in diversity, whose research is the problem of philosophy, is also the animating principle of philosophical medicine. But this cannot be appreciated by persons who are neither physicians nor philosophers.

The complete theory of practical specialism admits that a man may be a shining light in a subject "which interests him," yet a perfect idiot in another of equal importance to the patient. Now, the initial problem of diagnosis is the decision of the department to which the case belongs; and, on the above theory, the fate of the patient must be a matter of chance. If his case happen to fall on the competent side of the doctor he consults, well and good; but if not, it must fail of recognition. No fixed value can be attached to any symptom, when it is remembered that the lines of disease intersect each other in every direction.

Thus, does a young girl fall into a melancholy? The question would arise: Shall she be at once entrusted to the gynecologist on the suspicion of uterine disease, or to a hæmatologist for chloro-anæmia, or to the superintendent of an asylum as a case of incipient insanity, or to a friend of the family to bring about a thwarted project of marriage? If a woman has a pain in her back, how many physicians must be consulted before deciding whether this be due to muscular denutrition, or to uterine displacement, or to chronic nephritis, or incipient myelitis, or to commencing caries of the vertebra, or merely to hysteria? When a typhoid fever simulates general tuberculosis, or the reverse, should the diagnosis be made by the heart and lung specialist, or by the fever doctor? When a man falls down in an apoplexy, does his case belong to the neurologist, or to the specialist in diseases of the heart whence an embolus may have been carried, or to the practitioner devoted to gout and atheroma? Shall a children's doctor decline to perform an urgent tracheotomy because he is not a surgeon? or shall a physician tolerate irreparable delay in reducing a dislocation for the same reason? [1]

It is sometimes said that the conscientious specialist will be sufficiently trained in general pathology to recognize when a subject lies beyond his domain, and he will then, "in justice to his patient," hand him over to one of his own "eminent colleagues."

Dr. Barnes, who, of all gynecological specialists, most frequently deprecates specialism, thus illustrates the case: "A woman comes to him complaining of pruritus. Much to her astonishment, he examines her urine, because he retains enough

[1] We have within a few weeks seen two cases of irreparable injury caused by just this fact, and by the prolonged application of poultices instead of prompt operative interference.

knowledge of general pathology to know that pruritus may indicate diabetes. Finding sugar, he at once resigns the case and sends her elsewhere." This illustration represents a class of cases which do often occur, and where the specialist is really both competent and conscientious the case may be managed without further inconvenience to the patient than that of a double consultation. But—and this is a practical inconvenience of perhaps a low order for mention here—there is certainly no more, but rather less, guarantee for the honor of a specialist than of a general practitioner. The last is expected to take charge of the patient whatever may prove to be the matter with him. His interest, therefore, in ascertaining the exact state of things is identical with that of the patient. But the specialist knows he will only be entrusted with the case if he can prove that it falls within the limits of his own specialty. He is therefore always under a strong temptation to "make out a case," and for this purpose, if necessary, to rather avoid than to seek close scrutiny of the surroundings.

We hasten to recognize the fact that there are many specialists of honor as high and unsullied as could be claimed for the most upright physician. But we think the existence of the special temptation we have referred to can hardly be doubted, nor that this temptation is by no means always resisted. Apart from this purely practical consideration, it is to be remembered that such definite grounds of classification are more often absent than present; the specialist confronts the theoretical difficulty of not being quite sure what he is to exclude.

Another important inconvenience in the tendency to universal specialism is that the beginnings of disease are so often likely to escape detection. To consult a specialist, the patient will first wait until he is pretty sure he has the specialist's disease; thus, he must wait until this is rather well developed. Thus, too often no attempt is made to treat a chronic disease until it has become almost incurable, nor to make the precise diagnosis of an acute disorder until it has nearly killed the patient.

But the collapse into inefficiency of a general practitioner is not an adequate basis upon which to develop an accomplished specialist. Instead of either the one or the other, we obtain a confused, vague, cheerfully optimistic "family doctor," who relieves himself of responsibility for one organ in his patient's body

after another on the ground that it belongs to some "specialist," who, as long as symptoms are not importunate, declares that they will "pass away of themselves,"—instinctively dreading the recognition of their importance as the signal for a surrender of the case. Thus, epitheliomas are allowed to extend until they are ineradicable, and chronic pneumonia to eat out caverns in lung tissue unsuspected, and the child to limp from habit into a suppurating coxitis, and the melancholic to commit suicide while sent on a journey for change of scene.

In addition to the functions which may be unquestionably fulfilled by specialists with great advantage to the community at large, other claims are often advanced of, we believe, less validity. Thus, it is said:

1. That to specialists alone, or chiefly, is due not only the improvement of technique, but the discovery of the fundamental ideas which change the face of science.

2. That specialists are habitually engaged in life-long researches in the subjects of their specialty.

3. That, thus, the patients of a specialist must profit much more by his intellectual activity than can the patients of a general practitioner by his.

4. That, whereas a general practitioner can only have at best a partial acquaintance with the many diseases he treats, the specialist, in virtue of his wise limitation of observation, can know all about his.

5. Finally, that the establishment of specialities alone permits the accumulation of clinical material in definite and available masses.

The first claim might be contested *a priori* from the consideration of the evident necessities of the case. No idea in a specialty can be as fundamental or as original as that on which the specialty is founded, and this evidently must have been suggested by a non-specialist. Laennec was not a specialist when he practically discovered the principles of auscultation; his prolonged special application afterward was devoted to the consolidation and simplification and detailed establishment of his theory. Helmholtz was no oculist when he invented the ophthalmoscope; even his treatise on optics was written later. Czermak was not a specialist when he invented the laryngoscope. Orthopedics, perhaps, dates its modern impulse from the researches in locomo-

tion of the brothers Weber, who were physiologists. The principle of counter-irritation in joint diseases was established by Pott, a general surgeon of London; the still more important principle of rest was elaborated by Bonnet, a general surgeon of Lyon. The effective introduction into orthopedic surgery of resection was made by Sayre before he became an orthopedist. In gynecology the capital operation of ovariotomy was initiated, as is well known, by McDowell, a general surgeon, having been originally suggested by Hunter, than whom none of the great physicians of the eighteenth century was less of a specialist. It was the great surgeon Belpeau, and the author of a treatise on neuralgia, Valleix, who first called attention to uterine flexions and suggested pessaries. Dr. Sims had hardly become a specialist when he invented his speculum and contrived his operation for vesico-vaginal fistula, achievements which his long career has never enabled him to excel.

Modern dermatology is based upon anatomical researches, which may be, and often are, carried on by histologists who do not practise medicine at all,—hence could not be called practising specialists. The clinical researches of the French school, being conducted according to the theory of diathesis, were not and could not be made by physicians limited in clinical observations of skin diseases. The theory may be discarded; but the results of the impulse given under its influence remain. In neurology clinical specialism was first suggested by anatomy, and later by physiology. In no practical specialty is modern clinical observation kept more closely to these two fundamental sciences than in this. The principal facts and ideas have come from anatomists or physiologists, or from non-specialists, who have also furnished the chief clinical groupings. Bell's discovery of the double function of the roots of nerves was made in his capacity of anatomist; his discovery of external facial paralysis, in his capacity of general practitioner. Marshall Hall, Brodie, Abercrombie, Calmeil—even Broussais, with his "De l' Irritation et de la Folie,"—and a host of others, who were the early pioneers in this century in the study of nervous diseases, were not specialists, since it was indeed at that time not possible to be one. Nevertheless, many of their observations remain of permanent and fundamental value. The most eminent physiologists, who have contributed to knowledge of nervous diseases far more

than have simple clinicians, have not been specialists in the physiology of the nervous system. Magendie, who divides with Bell the honor of the discoveries in the spinal roots of nerves, wrote two volumes on the "Physics of the Animal Organism." Bernard is as distinguished for his composite researches in diabetes (to go no further) as for those on the vaso-motor system. Schiff, who distinguished the paths in the cord for different sensory impressions, has written a treatise on digestion. Neither Türck nor Bouchard were practical specialists when they established the fact of descending degenerations; nor was Waller when he made the famous experiment which has served to explain these morbid processes. Brown-Séquard's researches in epilepsy were made at the very beginning of his career, and not when he had become a specialist. The clinical groups of locomotor ataxia and pseudo-hypertrophic paralysis were established by Duchenne, whose specialty was not nervous diseases, but faradic electricity, and originally, in its application to orthopedics. Exophthalmic goitre has been discovered by Basedow, a sagacious general practitioner; and the same is true of Addison's disease. Gubler, the first to point out crossed paralysis, was never a specialist; indeed, his essay on the hepatic lesions of hereditary syphilis is as famous as any that he has written. Sir William Gull's and Stanley's observations on paraplegia from renal calculus initiated research into "reflex paraplegia." No one could suppose them to be specialists.

Another class of examples is offered by writers who had become specially identified with neurological practice before publishing the treatises now recognized as authoritative, yet who, before this, had achieved distinction in other directions. Thus, Griesinger's now classical work on psychiatry was preceded by an only less famous treatise on infectious diseases. Leyden, before writing two volumes on diseases of the spinal cord, had published a valuable monograph on icterus. Nothnagel's admirable clinical contributions to the problem of cerebral localization, and his less admirable experiments on the brain, cannot efface recollection of his hand-book of therapeutics—on the whole, the most valuable extant on the subject. Charcot began his studies in neurology by general studies on the diseases of old age. He was stimulated by the practice of no specialty, but simply utilized the neglected pathological materials accumu-

lating in oblivion at the Salpêtrière. Only recently, moreover, Charcot has published a series of lectures on the pathology of the liver and of the kidney; and his description and analysis of the lesions of broncho-pneumonia have thrown new light on a subject supposed to have become hackneyed.

These examples, selected at random, do not of course exclude the clinical discoveries or inventions which have been made by practising specialists, and in a manner which indicates that they were the direct outgrowth of their special clinical experience. In neurology, Westphal's discovery of the tendon reflex symptom; in gynecology, Emmet's operation for lacerated cervix, are typical examples of this class. The fact that Hitzig, whose discoveries on the motor irritability of the cortex have had such an enormous influence, has been for a long time the superintendent of an insane asylum, is not an example of the influence of practical specialism. His researches were purely physiological, and were suggested by physiological considerations, which clinical observations might confirm, but did not suffice to originate.

We think the cases quoted are sufficient to demonstrate that indefinite repetition of clinical experience is never of itself sufficient to suggest new ideas; that a life-long specialism in no wise predisposes to discoveries, and still less is essential to their achievement; that in a large number of cases, if not the majority, the consecration to a specialty has followed, and not preceded, the discovery which has achieved the reputation of the specialist, and has fascinated him, perhaps for ever, with the subject. But it is always genius which invents; special application can only improve; it then remains for culture to appropriate.

Our limits compel us to be brief with the three remaining propositions. In regard to the second claim, namely, the life-long researches supposed to be carried on by practising specialists, we would call attention to a fact usually overlooked. It is that for every mind, in regard to every subject it studies, there exists a saturation point of suggestiveness, which is not exceeded by enforced prolongations of attention. It is very useful for a person to pursue a subject, so long as it continues to yield him ideas; very useful to practise a technique, until it be sufficiently mastered to meet all difficulties of execution. But afterward there remains no intellectual advantage in persistent adherence to the same line of thought. There are personal, often pecuniary

advantages; there is profit gained from an acquired reputation and previous labors. But this, however legitimate, is a very different thing from continued progress in science, or indefinite improvement in care-taking of patients, such as is generally assumed.

Again, the practical specialist does not, fortunately, often select only one disease, but one organ, or presumably associated group of organs. Now cases of the same disease in different organs are apt to present many more points of resemblance than do cases of different diseases in the same organ. There is much more analogy between uterine cancer and epithelioma of the lip than between uterine cancer and uterine flexions. The study of the pelvic curves throws no light on embryology, although both subjects are assigned to the obstetrician. Uræmic peritonitis is better understood by study of septic peritonitis than of renal calculus. Epilepsy has much less resemblance to the systemic forms of myelitis than to the eclampsia induced by acute hemorrhages, and so on.

Practical specialism only enforces attention to clinical observation: analysis of this, on the basis of any special science, is as optional with the specialist as with the general practitioner, and as liable to be neglected. Many good specialists are purely clinicians; many others, really distinguished in some branch of science connected with special disease, are quite innocent of others. Perhaps from few experts in consultation would we expect familiarity with such a monograph as Bert's on respiration, or with the complex laws on diffusion of gases. It would not be difficult to name neurologists distinguished in experimentation, but who have never mounted a section of nerve tissue for the microscope. It would not be impossible to cite skillful surgeons, most ingenious in mechanical contrivance, who are unaware of the pathological anatomy of the tissues they divide or remove.

Great as are the difficulties arising from the great increase in the mass of knowledge, there are many palliations. The perfected machinery for sifting, analyzing, classifying, and sorting this knowledge, renders it ten times as accessible and comprehensible as was formerly one tenth part as much. Many general principles have been established, which link together, in lucid unity, hosts of details, once unconnected, unintelligible, and hence most difficult to remember. The classical body of

doctrine in medicine, whose possession is essential to the practice of medicine (*secundum artem*), is really more accessible to-day than at epochs when some narrow system professed to crush it into a portable nutshell. Finally, the advance of science and of scientific method exacts, that who would claim to contribute to further progress must concentrate himself much within the limits of any conventional specialty. No one disease, no one organ may be compassed by a single observer: happy he who may, by laborious research, contribute to the solid establishment of a single detail of the truth. For such work it is, theoretically at least, as easy for the general, as for the special physician to withdraw a certain portion of his attention from practice. Neither can hope that his research can benefit more than a small proportion, if any, of his own patients. The one must, as much as the other, depend on the collaboration and unconscious coöperation of a thousand workers. For both, are needed not only clinical observations, but the mental ability to utilize observations,—a mental training in the art of handling large masses of ideas. For both, if we may judge from European examples, the personal experience to be gained in private practice is insufficient; to both, should classified hospitals be open as the true field for pathological study.

SHALL WOMEN PRACTICE MEDICINE?[1]

THE continually renewed discussion, on the part of society, concerning the sphere, capacities, rights, functions, duties, and allowable occupations of women may well seem, from some points of view, rather ridiculous. We may justly ask why women require so much more discussion and preachment than men; and may even decide that the argument is largely superfluous, and the sermon often impertinent.

Further consideration, however, discloses several grounds of justification for this social habit, from which, in any case, it is quite impossible to escape. In the first place, women, as the most malleable part of the social organism, are destined to receive the first, and also the most lasting, impress of prevailing social opinions. They transmit—the phrase is becoming classical—the organized experience of the race. The least change in such experience affects them especially, and hence they must bear the special brunt of the criticism upon it.

In regard to the particular subject we propose briefly to consider, social opinion is of very real importance. Success in a professional career necessarily depends, to a large extent, on the taste of the community. There must be a readiness to consult women physicians; a willingness to educate them; a sufficiently wide-spread desire on their part to be so educated. If the social prejudice be very strong, no young woman will dare express the wish to study medicine. Should the vagrant fancy arise, it will be promptly checked, as something eminently improper,—like going on the stage, or dancing on the tight-rope at a circus. That considerable numbers of women do now study medicine and support themselves by its practice, is itself a proof that the prejudice of thirty years ago has somewhat abated. Women are admitted,

[1] Reprinted from the *North American Review*, January, 1882.

in America, to the State universities of Michigan and of California, and sustain, moreover, three separate schools: one in Philadelphia, one in New York, one—the youngest—in Chicago. In Europe, they study at the universities of Paris, Zurich, Berne, Upsala, Leyden; have a separate school at St. Petersburg, and are admitted to examinations for degrees at the University of London, and also at Dublin. They are members of various medical societies, contribute to various medical journals, conduct hospitals, perform surgical operations, build up practice, and in other ways seem to conduct themselves and to be treated like other members of the medical profession.

Yet discussion still continues, and although the once continuous opposition has become intermittent, its crises are perhaps rendered more noticeable on that very account. The centennial meeting of the Massachusetts Medical Society was agitated by a renewal of the controversy concerning the admission of women physicians. Their cause found vigorous champions, but was defeated, when an equally vigorous opposition supported the majesty of precedent, by the tactics of parliamentary maneuvering. The siege at the gates of Harvard, destined to be as memorable, we believe, as that formerly laid against Thebes, is still maintained. The echoes of the fierce battle waged in the University of London have scarcely died away; a few years ago, the Society of German Naturalists, meeting at Berlin, voted to "purge itself of the presence of women"; and only last summer, public attention was called to the formal exclusion of women from the International Medical Congress, at its first meeting held in England. The measure, it is said, was taken in obedience to the wishes of the Queen, and certainly to those of the court physician, Sir William Jenner.

All innovations excite opposition. But it is difficult to account for the peculiar bitterness of the opposition which has been manifested to the admission of women to medicine, when it is remembered that this admission is no innovation at all. Women practiced freely in medicine so long as the practice of medicine was free, and entrance upon it was decided merely by natural taste for dealing with the sick and ministering to their infirmities. When, however, instruction in medicine began to be systematized, when universities took charge of it, and legal standards of qualification were established, women were excluded, because,

at the time, no one thought of them as either able or willing to submit to the new conditions imposed. The monastic discipline out of which universities had emerged still molded their etiquette sufficiently to render them inaccessible to women. The women themselves do not appear to have thought of presenting themselves as candidates for a university education. Thus, in the onward current of progress, the women physicians of the Middle Ages, or, in France at least, of all the centuries preceding the Revolution, were dropped on the bank. Women are now merely endeavoring to reënter the stream, by adapting themselves, whenever they are allowed to do so, to the changed conditions of things.

In this effort, the most serious obstacles to be encountered are not always the most real ones. In this, as in everything that women do, the question of capacity is often outranked by the question of taste. Whether woman, with all her organic imperfections on her head, can be theoretically supposed capable of the study and practice of medicine; whether, which is quite a different question, there actually exist any number of women whose capacity in this direction has been fairly tested and demonstrated, —these are interesting subjects of inquiry. But the most completely affirmative answer to such inquiry might still leave unsettled a question of much more importance for that large class of people whose convictions and actions are under the permanent domination of their tastes. These ask not, "Is she capable?" but, "Is this fearfully capable person nice?" Will she upset our ideal of womanhood, and maidenhood, and the social relations of the sexes? Can a woman physician be lovable; can she marry; can she have children; will she take care of them? If she cannot, what is she? "*Qu'est ce qu'une femme,*" said a French journalist in this connection, "*qui n'est ni épouse ni mère?*" "God," declared a Boston physician, well versed in the counsels of Providence, "never intended women to practice medicine." Hence the inference that piety, if nothing else, demanded the exclusion of women from the Massachusetts Medical Society.

It is from the peculiarity of the conditions involved, that the handful of women now engaged in the practice of medicine may be considered in any way to affect or endanger existing arrangements or social ideals. Thousands of women, from manifold causes quite extraneous to medicine, remain celibates all their

lives; yet no one reproaches them for refusing the duties of wife and mother. Thousands of women earn their living by non-domestic labor; one profession, that of public teaching, practically thrown open to women only during the last half-century, is already thronged by them. Yet no one feels that the foundations of society are therefore liable to be overthrown. What is it in the profession of medicine which excites, at present, such different feeling and such bitter prejudice?

There are several things. In the first place, the profession of medicine has always been subjected to popular misconceptions, and the odium due to these is necessarily shared by the women who aspire to be physicians. Again, by a social fiction, it is assumed that the usual employments now sought by women are to be filled by them only while waiting for marriage, or as a resource in widowhood or desertion. Even such professional work as teaching is expected to be laid aside after a few years, and there is much, at least in the primary grades of teaching, to make such interruption rather desirable. But the profession of medicine must be chosen deliberately, and not at hap-hazard; from a strong and genuine taste, and not from the mere pressure of economic necessity; it must be seriously prepared for in youth; must be entered upon at the age at which at present many women marry; does not yield its best returns until full maturity has been reached; must be adopted, therefore, if at all, for a life-time. Hence is required either an accidental celibacy or a deliberate renunciation of marriage for the sake of medicine, such as is not dreamed of in regard to any other work; or else such an adjustment of domestic claims as shall render them and the practice of medicine by married women mutually compatible.

But further, apart from the special odium attaching to medical knowledge, the assumption of capacity on the part of women for any knowledge which leads to first-class responsibilities offends the average social ideal. Again: The idea of mental training as a means of developing force is rather new to the world in any aspect. It is practically almost unthought of in regard to women, who are habitually estimated by the measure of their native, untrained capacities. This is seen to be inadequate for the responsibilities of medical practice.

To consider a little in detail the foregoing topics. The assertion that medicine and physicians are permanently and pro-

foundly misunderstood by the public may not at once be accepted. Yet, it is certain that, despite the familiarity of his presence and appearance, the laity know less about the doctor than about any one else with whom they have to do. They cannot understand why he wants to dissect, or to "vivisect," or to make post-mortem examinations; why he stickles for a punctilious etiquette; why he is fascinated by repulsive objects; why he can find fathomless mysteries in the commonplace miseries which they have to endure; and how, by any process of reasoning, the recondite connection between these mysteries can be detected and made clear. The handling of familiar things in an unfamiliar way is a process inevitably bewildering to the uninitiated spectator. There is something uncanny about it. Moreover, the human body has ever been esteemed sacred. From the Egyptian embalmer down, those who have dared to intrude upon its mysteries have been branded as profane. When, from the pressure of evident necessity, the profanity has been tolerated, the toleration has only half-repressed a shuddering horror at the sacrilege. The violent popular excitement recently aroused in fox-hunting England against physiological experiments—with such effect that they have been practically forbidden by legislation—recalls the still more violent agitations in the last century against "body snatching," and the legislative repressions of anatomical studies. Mr. Tennyson, in one of his latest poems, draws a caricature of the most humane of professions in the person of a "red-bearded" student from "the hellish schools of France." In 1794, Mrs. Shelley, in her romance of "Frankenstein," stigmatized the sublime search after the origins of life as "dabbling in the filthy secrets of the grave." The same sentiment really animates the modern poet-laureate and the wife of the elder poet; although in recent times exquisite experiments have somewhat redeemed the theme of the spontaneous generation of life from the realm of "filthiness," and the "anti-vivisection" prejudice drapes itself in the pretext of philanthropy. But at bottom the feeling is identical. Life is a mystery; the attempt to penetrate mysteries is a sacrilege; and terror of the awful, unknown consequences of sacrilege is quite sufficient to overpower the reasonable apprehension about intrusting the care of sick bodies to persons who have been forbidden to learn anything about them.

Now, the introduction of women into a sphere regarded as at

once dirty, horrid, and irreverent certainly shocks many of the "finest sensibilities of our nature." The feminine university founded by Tennyson's lovely Princess had, among all its schools, "not one anatomic." She could not bear

> "——to ape
> The monstrous male, who carves the living hound";

and only in the spirit of the sublimest self-sacrifice could she, fearing casualty, be induced,

> "——through many a weary month,
> To learn the craft of healing."

The poet does not seem to doubt her capacity for mastering this wearisome business, but evidently feels that the Princess would have been alienated from poetic sympathies had she found the task other than repulsive—had she delighted and gloried in it as a real physician must do. In this estimate, he strikes the key-note of average popular sentiment.

That the study of the mechanism of the human body is not mere dirty work, but one of the most sublime occupations; that mysteries are not sacred, but embarrassing masses of ignorance destined to be dispelled; that the sensuous disgust attendant on anatomical and physiological research can be, and is, completely consumed in the divine flame of an idea; that human life is more precious and more deserving of reverence than any of the accidents, physical or social, by which it is environed—these convictions have been steadily pressed against the inert minds of the unreasoning multitude, until they have at last secured for themselves toleration, if not acceptance. The odium attaching to the study of medicine by women must be overcome by similar means. The charge of "unsexing themselves" by the acquisition of the particular kind of knowledge required in medicine is, after all, less formidable than that of "dehumanizing themselves," which, in one form or another, has so often been brought against men for the same thing. With those whose beliefs are not a matter of reason but of habit, the mere repetition of a fact until it becomes habitual is sufficient to insure acquiescence. This circumstance goes far to compensate the inconvenience of the prejudice engendered by the mere fact of unfamiliarity.

Touching closely upon the universal prejudice which is primitively rooted in the terror of sacrilege, comes another, which, at

the present day, is held almost exclusively in regard to women. It is often said that the work of practicing medicine is necessarily so coarse and disagreeable, that none but coarse and disagreeable people are naturally fitted for it; or, if others engage in it, they must inevitably deteriorate to an inferior personal and social level.

Now, the people who advance these statements have often themselves been sick—have had, therefore, frequent personal intercourse with physicians. It is, therefore, pertinent to inquire whether these delicate ones have always found their own physicians to have been rough-shod brutes, or whether they consider that the task of ministering to their infirmities in any way necessitates coarseness and harshness? The tacit answer to this inquiry is, we believe, that refined people would never do anything so eccentric as to consult a woman physician. She must perforce "go about among all sorts of people," pick up her practice where she can, and the process of "going about" is often alluded to as if it implied carrying a revolver, or seeking the escort of a policeman.

Of all the social bewilderments with which this question is befogged, this is, perhaps, at once the most ridiculous and the most exasperating. It is impossible to imagine a sphere in human life, with the exception, perhaps, of the artistic, in which delicacy —mental, moral, and even physical—is more essential than in that of the physician. The preservation of decorum, the maintainance of suitable reserves, the just balance of rights, the quick perception of feelings, all these are the natural correlatives of the deft physical touch, of the intellectual subtlety, which should, and which does, characterize a true physician. What is there in all this incompatible with the classical, not to say conventional, ideal of feminine character?

There is another consideration more excusably overlooked. It is impossible to be a physician on the basis of personal sympathies alone. If the interest in the disease be not habitually greater than the interest in the patient, the patient will not profit, but suffer. He may gain a nurse, but he loses a physician. Now disease, even more than death, tends to level distinctions. It diminishes the social value of those who have any; but, on the other hand, it invests with an otherwise unattainable interest those who are quite lacking in social charm—the stupid, the

vulgar, and even the vicious. The physician is, indeed, the only person who can "go about among all sorts of people," unbored and uncontaminated. When the priest does the same thing, it is because, as far as may be possible, he imitates the bearing of the physician.

The only possible excuse for this wide-spread assumption, that women physicians must be inferior to men in personal refinement and social culture, may be found in the conditions under which women have hitherto been obliged to study medicine. The obloquy heaped upon women students of medicine has been so great that many women of refinement have been repelled from a pursuit to which their natural taste inclined them. Conversely, many women have entered upon it without taste or understanding, but merely attracted by the flavor of notoriety and the enjoyment of something slightly turbulent and very eccentric. Not these ignorant women, but society, are to blame for the opportunity accorded to put forth their absurd pretensions. A Nemesis waits upon the rejection of just demands. The refusal to admit to a disciplined education and to submit to suitable tests the women who were really fitted for both, has merely resulted in the rather extensive education of the unfit; and this has often been carried on in the very least suitable manner which human ingenuity could devise for the purpose.

Considerations of delicacy have been urged, as is well known, in a special manner, both for and against the admission of women to medicine. On the one hand, the association of women with male students in professional schools and medical societies, has been denounced as an indelicacy which rather more than borders upon immorality. On the other hand, the treatment of female patients by male physicians—especially in a certain class of diseases—is shown to involve a straining of delicacy which cannot but be most undesirable, even when it is submitted to as inevitable. In the most populous quarter of the globe, in all the countries of Asia, it is known that such submission is not considered inevitable—is, indeed, not allowed. The alternative is invariably accepted of leaving the female half of the community entirely unprovided with medical attendance for any disease whatever.[1]

[1] To the Editor of the Pall Mall Gazette.

Sir: The October number of the "Indian Female Evangelist" supplies an interesting piece of evidence on the disputed point as to whether properly

No hard names which have ever been heaped upon the women who want to study medicine can exceed those once lavished on the presumptuous men who first forced their way into midwifery. As late as the seventeenth century, even at the time that Chamberlain was inventing the forceps, the term "man midwife" was as much a term of reproach as that of "female physician" often is at the present day. The feeling of delicacy, permissible, even imperative in itself, was compelled to yield to the still more im-

---

educated medical women would or would not be acceptable to the native ladies of India. It appears that the Maharajah of Punna, in Bundelcund, applied to Miss Beilby, a female medical missionary at Lucknow, to treat his wife, who had long been suffering from some painful internal ailment. Miss Beilby spent some weeks in attendance upon the Maharanee, and happily was able to effect a cure.

When the time of her departure from Punna arrived, she was desired to present herself at the palace to take leave of her royal patient, on Wednesday, the 13th April last. The Maha-Rani received her in her private room, and almost immediately dismissed all her attendants and ladies, so that she might be quite alone with her. The Maha-Rani then said she wished Miss Beilby to make her a solemn promise. Without knowing what it might involve, she was reluctant to do this, but at length the Maha-Rani said: "You are going to England, and I want you to tell our Queen and the Prince and Princess of Wales, and the men and women in England, what the women in the zenanas in India suffer when they are sick. Will you promise me to do this?" She explained that it was no social change in their condition she sought, but relief in their cruel sufferings. She charged Miss Beilby to give this message *herself* to the great Queen of England; not to send it through any other channel, but to take it *herself*, or her Majesty would think less of it. Miss Beilby represented to the Maha-Rani the difficulty she would have in getting access to the Queen—that with us it is not as in the East, that any one can go to the palace and lay a petition before the native sovereign. Besides, she told her she hardly knew what good it would do if she could do as she wished, and take her message to our Queen. The Queen could not *make* lady doctors, or *order* them to go out. It was not in the power of even the great Queen of England to do this. "But," said the Maha-Rani, "did you not tell me our Queen was good and gracious, that she never heard of sorrow or suffering without sending a message to say how sorry she was, and trying to help? Did you not show me a picture of a train falling into the sea, where a bridge broke, and did you not tell me how grieved our Queen was? Well, it was very sad those people should have been killed, but our condition is far worse; if you will only tell our Queen what we Indian women suffer when we are sick, I am sure she will feel for us and try to help us." Miss Beilby felt she could no longer refuse to promise to convey this message, if possible. The Maha-Rani next bade her write it down at once (giving her pen, ink, and paper), lest she should forget it, and added, "Write it small, Doctor Miss Sâhiba, for I want to put it in a locket, and you are to wear this

perative claims of superior knowledge and capacity. If this has ever been accomplished, it is not doubtful that a legitimate feeling of delicacy—as that which makes many (not all) women dislike to be treated for at least uterine diseases by a man—should, if once thoroughly reënforced by legitimate confidence in feminine skill, overpower the quite superficial ideas of delicacy in regard

---

locket round your neck, till you see our great Queen and give it her yourself. You are not to send it through another."

On reaching England, Miss Beilby communicated with some of the ladies about the Court, and on July 13, 1881, the Queen received her at Windsor Castle:

Her Majesty listened to Miss Beilby's statement with great interest, asking many questions, and showing the deepest sympathy. Turning to her ladies, she said: "We had no idea it was as bad as this; something must be done for these poor creatures." The Maha-Rani's locket with its message was given to the Queen, and Her Majesty entrusted Miss Beilby with a message in reply, which was intended for the Maha-Rani alone. But the Queen also gave Miss Beilby a message which might be given to every one with whom she spoke on the subject of the poor suffering Indian ladies:—"We should wish it generally known that we sympathise with every effort made to relieve the suffering state of the women of India."

We fear the Maha-Rani would after this be disappointed if she were told that three weeks later the medical women of Europe and America were excluded from the International Medical Congress held in London last August, and that this exclusion was effected by the Queen's private physician, threatening the Congress with the loss of the Queen's name as patron if medical women were admitted. If this were anything more than an unauthorized application of the influence of royalty, it would be desirable for the Queen to remember that it will not assist in relieving the suffering state of any of her Majesty's subjects to prevent their medical attendants from keeping *au courant* with every advance in the knowledge of the complex art of healing, and that it is not true that a very much less educated practitioner than those who desired to attend the Congress would be good enough for India. The fact of the skin of the patient being some shades darker than our own does not, as some people seem to imagine, simplify alike the physical organization and the abnormal conditions of the body, and if good medical women are wanted for India, they must receive as thorough a training as the best medical schools in England can give to men. The recent successes of the students from the London School of Medicine for Women in the Honor List of the London University show that in this school, at any rate, the education given is good and thorough, and we hope her Majesty will in due time have the gratification of knowing that many medical women who have been trained there are at work in India and England in relieving the sufferings of her subjects.

I am, Sir, your obedient servant, B.

October 25.

to co-education in medicine. We call these ideas superficial, for they only represent further misconceptions of the mental attitude of true medical students. The scope of the subjects studied is so immensely wider than the public can imagine; the mass of its details so much greater; the intellectual aspect so different; even the material conditions so changed,[1] that it is quite impossible for any one on the outside to judge of the form of feeling likely to be excited by the actual circumstances within.[2]

From all this series of misconceptions to which women are exposed in common with men physicians, and, for many reasons, more conspicuously than they, it would seem as if members of the profession should naturally be exempt. "It is an ill bird that fouls its own nest"; and it seems scarcely credible that any physician who loves and honors his calling as it deserves, should dare to pronounce it too coarse or too hardening a pursuit for women. Whenever this has been done, the argument is necessarily insincere. It is like the outcry of school-boys when their sisters beg to be allowed to play ball with them. "Go away! You are a girl! *Girls* don't play ball!" The school-boy is usually unable to enforce this brief but effective dictum by dissertations on the difference in the form of the clavicle between the male and the female, and consequent inferences as to the necessary inefficiency of girls in the art of pitching and catching. Grown to manhood, however, he learns to justify his opinions by formidable weights of erudition. These arguments vary from age to age, and to-day the fashionable one is drawn from natural history. By laborious researches into the comparative weight of the brain,[3] the strength of the muscles, the depth of the respiration, the powers of digestion, the richness of the blood, it is established that the typical woman, wherever she appears, must be an inferior animal to the typical man, wherever he may be

[1] As in the dissection or post-mortem examination of dead bodies.

[2] Not to interrupt the course of the text, we would here note that schemes of co-education which, in some shape, are really essential to the proper professional education of women, are always compatible with isolated instruction on the very few special subjects where the association of young men and women students might be an embarrassment. But these topics occupy, after all, a very small part of medicine.

[3] It will not be forgotten that the latest tables of Bischoff give the proportions of the brain to the weight of the body as 1 to 36 for women, 1 to 37.5 for men.

found. The rapidity with which this abstract conclusion is applied to such a concrete problem as the capacity of women for the practice of medicine is amazing. Were the feat performed by feminine reasoners, it would, no doubt, be cited in proof of the hasty generalizations of the shallow female intellect. But we remember the fable of the wolves and the shepherds!

The logical inference from such data as we have quoted, precisely because they have recently been re-adduced in the argument about women physicians, can only apply to the relative positions of men and women in the social organism. We might infer, if we admit the validity of such researches and the reality of their statistical value, that the highest, and weightiest, and greatest amount of effective work must always be performed by the masculine half of the race. But it by no means follows that the work of the medical profession lies on this loftiest plane, and, consequently, the argument in question has nothing to do with the matter at issue.

Here is the point which, so far, we have hardly ever seen distinctly appreciated, namely,—that, as the gamut of human intelligence goes, a third-class intellect is quite sufficient to make a first-class doctor.

This will be clear when it is remembered that by first-class intellect is meant that of creative genius; by the second, that of inventive talent; by the third, the mind possessing the power of generalizing, adapting, and coördinating what others have created, discovered, or invented. If we take Newton as an illustration of the first class, Faraday of the second, Trousseau of the third, it will be evident that the great mass of even our first consulting physicians occupy a lower rank still. On this calculation the trustworthy but undistinguished family physician, the sheet-anchor of many homes, must modestly acknowledge that he holds only the fifth place,—often, indeed, not that!

Surely the natural history argument, which gives the abstract estimate of women's capacities as so little lower than those of man, cannot be used to consign her to the perdition spread out below this fifth circle! Were it necessary to apply the interpretation, it would be that if all men were Newtons, no woman would rise higher than Faraday. If the mental development of the race had paused at the level of Faraday, no woman could claim more than the erudition of Trousseau, and so on.

The absurdity and uselessness of such a discussion is fully paralleled by that of the innumerable discussions which have been solemnly sustained on this basis. This is our excuse for pausing to consider it.

Some years ago, Huxley took occasion to say in public: "No scientific man, well acquainted with the quality and quantity of the intellectual work actually performed by the average medical practitioner, could doubt that any vigorous girl could be trained for the same."

The intellectual work required of physicians is of two kinds. They *must* learn an art, and become experimentally skilled in its various applications. And, during the exercise of this art, they *may* collect data which shall contribute to the advancement of the science upon which the art reposes.

The relation between these two branches of work much resembles that which exists between the art of musical execution and the science on which depends musical composition. Now, it is well known that abilities in these two different departments exist in no necessary proportion to each other in the same person. Brilliant performers are known whose compositions are thoroughly mediocre; the most profound musical writers may be relatively inferior in the technique of figuring. In the conservatory at Stuttgart, we have been told that the female pupils are restricted to the study of execution, and receive no instruction in the principles of harmony or theoretical music. They are not expected to compose.

The mental powers involved in the application to concrete problems of the principles of so great and complex an art as that of medicine, may easily seem to the outsider to be identical with those concerned in scientific research. This is not, however, the case, and therefore objections made to the education of women as physicians because, without education, they have made no important scientific investigations, should fall to the ground by their own weight. Such objections, if maintained, must rule out of practice the great majority of successful practical physicians.

Another consideration: Persistent innovations are rarely one-sided. When new claims are made, we may be sure that they have a foundation in facts. It is so with medicine. Its modern development renders it more accessible to women. Thus, the discovery of anæsthetics has thrown open to women

almost the entire field of operative surgery, from which, formerly, purely physical disabilities must have excluded them. This is a branch of medicine capable of being taught with great precision: and, accordingly, we find that a taste for surgery develops rapidly among women wherever they can obtain for it the requisite personal training.[1] Again, the immensely greater attention paid in modern times to the chronic diseases of all the organs of the body, opens not one but many fields where care, patience, solicitous observation, detailed attentions, are more required than the prompt courage supposed to be necessary for startling emergencies. But, finally, the majority of emergencies cease to be startling, because, owing to the greater precision of medical knowledge, they can be far more often foreseen and far more often coolly analyzed and interpreted.

In a word, whatever tends to perfect the art of medicine, tends also to render it more susceptible of being taught with positiveness, hence more accessible to persons susceptible of training, but liable to be deficient in originality and initiative. It is the achievements of masculine genius which, in medicine as in other departments of life, facilitate the work of women. To what extent real genius for medical science may develop among women remains to be seen. The conditions for such development do not yet exist. Medical training is, in America, everywhere extremely imperfect, and in regard to women, the imperfection becomes more obvious on account of the habitual defects in their ordinary education. They are, as has been already said, deficient in mental initiative; the deficiency is not counteracted, but aggravated in the great majority of cases by almost all the influences to which they are subjected from their cradle upward. Few agree with Mr. Morley "that there is probably nothing which would lead to so rapid and marked an improvement in the world as a large increase of the number of women in it with the will and capacity to master Newton as thoroughly as she (the Marquise du Chatelet) did."[2] Everything in ordinary life is opposed to the thorough mastery of anything by women. The study of medicine necessitates, in this respect, an entirely new departure.

[1] Dr. Van de Warker says that we are yet to see the female ovariotomist. We know of at least seven ovariotomies performed by women, of which five were successful.

[2] Life of Voltaire, p. 100.

Experience shows that this is not difficult to effect wherever women students are submitted to an authoritive and imposing discipline—as in the European universities to which they have been admitted. But the self-enforcement of such a discipline is necessarily rare. The capacity of women for purely mental initiative is often at present encroached upon by the severe struggle with practical and pecuniary necessities to which so many of them are subjected. The self-denial, energy and pluck, ingenuity and perseverance of hundreds of women students would make, if published, an heroic record. Through poverty, opposition, ill health, often with insufficient daily food, —often compelled to work, in addition to their studies, to earn their daily expenses,—these women struggle on uncomplaining. They are obscure, unknown—often remain so; often fail from attempting the impossible, yet, oftener than could be imagined, succeed at least in rising to the standard which is established for them. It remains, perhaps, for another generation to do more.

From what has been just said we certainly would not have it inferred that we argue the necessity of accepting women as an inferior grade of practitioners, to be tolerated in trifling ailments and to be set aside in serious illness. Our argument is simply that, at the present stage of medical development, the mental powers exercised in the treatment of the most serious illness lie, both as to quality and quantity, within the range of the theoretical estimate now generally made of the intelligence of women. These powers are apt to seem more extensive than they are, because, from the nature of the case, they usually come into play in the presence of persons unqualified to criticise them. So long, however, as the physician confines himself to the application of the rules of diagnosis and of treatment contained in his art, his work, however judiciously and skillfully performed, must, as an intellectual performance, be ranked as second-rate. To say, therefore, that the intellectual capacities of women are only second-rate by no means excludes them from the most responsible duties of practical medicine.

Again, chased from the first assumption, the determined prejudice finds refuge in a second, and we are confronted by the assertion that women physicians must be lacking in the normal qualities of self-reliance, steadiness of nerve, self-control, etc. Here again, we believe, the fallacy lies, first, in taking for

standards of comparison women quite untrained for the work; second, in misunderstanding the effect of knowledge in dissipating the alarms principally excited by mysteries. To be firm, self-reliant, and steady in dealing with the friends of the patient is no harder for a woman involved in medical than in other responsibilities. To assume the burden of such responsibilities in regard to the patient requires the ability to say: "I know the condition of this patient at least as well as any other person to whose opinion I could have access. I am also acquainted with the nature and extent of the resources which the art of medicine at present possesses for such a case. It is my business to apply those resources with all possible care and diligence, and to await the result."

This is not the frantic "wrestling for the life of the patient" which figures in popular imagination, but it is the sober truth. It is not the language of an excitable person, bewildered in a dramatic situation, but that of a well-balanced intelligence, thoroughly trained for the work which it has undertaken.

If a really first-class intellect be often rather out of place in the practice of medicine, a high degree of vitality, of organic vigor, is certainly needed. Now, it is not usually recognized to what an extent the organic vigor of women is naturally destined to be increased by child-bearing. The prevalent American notion is that maternity is the signal for an inevitable collapse of all mental and physical powers. The reverse is certainly intended in the scheme of Nature. The key-note to the difficulties of the position of women in regard to the achievement of distinction in any form of work, lies in the fact that the degree of their physical, and probably, therefore, of their mental, development before child-bearing is always rudimentary, relative to that attainable after it; while, nevertheless, the risks, duties, and social consequences of maternity tend to so completely absorb this increased vitality that none remains over to be expended in external work.

It is foolish to overlook or to dispute this fundamental fact. But it is equally useless to insist upon it, as in itself sufficient to decide the social destinies of woman.

The "social consequences of maternity" vary indefinitely with the social class. Immense numbers of women are compelled, by the most inflexible economic conditions, to work as hard, in factories or elsewhere, after marriage as before. "If," says Simon,

"the family can only be supported by three francs, and the man can only earn two, there is no alternative but for the woman to labor to secure the remaining franc." The range of non-domestic industry rises, without changing its essential nature, from the level of the European factory operative to that of the American farmer, where the share of the married woman in the conduct of the farm is considerable. Marriage cannot be said to withdraw from non-domestic industry the majority of women, but only to increase their burdens, and set them to work at a disadvantage.

On the other hand, in another immense class, or, rather, series of classes, the amount of work performed by women in the discharge of household duties is fully equivalent to the amount of non-domestic industry performed previous to marriage. The difference lies in the arrangement; and it is claimed that this facilitates the duties of maternity, and care of children, while any non-domestic labor must disastrously antagonize these.

Finally, for much the smallest, but also the most influential because the most distinctly articulate class, marriage means, or is expected to mean for the woman, liberation from any definite industry. The work of the household is performed by servants; and the funds are entirely supplied by the external work of the husband. This, indeed, in theory; in fact, in all dense social communities, and there especially in professional circles, the wife is often expected to contribute an essential quota toward the maintenance of the household, by means of an inherited fortune or of the dowry received from her father. As it is evident that physicians must come, not from the so-called laboring classes, but from those where the married woman either works in the house or does no real work at all, the practice of medicine by married women becomes involved in the following problem: To so arrange, at least certain forms of non-domestic labor, that even a married woman should be enabled to engage in them if her taste so inclined, either in preference to the domestic work which she would otherwise be compelled to perform as a substitute for a dowry, without which she might be compelled to remain unmarried, or as a substitute for an elegant leisure, which, to an energetic temperament, is often a refined torment.

On the theory that work is a mere personal hardship, to be evaded whenever possible, to be sought only for an indispensable pecuniary return, and always, even in our democratic coun-

try, implying for women a faint social disgrace, there is not likely to be any enthusiastic support among married women of professional or other work to be performed by any of their number. This is why the frequent denunciation of such work by fashionable women is always open to suspicion. On the broader theory, that the amount of work to be done in the world implies a collective fund of activity, to which all human beings may lawfully desire to contribute, each freely choosing such portions of it as are most suited to his or her special capacities, the matter assumes a different aspect. Whatever real difficulties may lie in the way, the one at least should disappear which is created by the half-avowed dictum: "No woman has any right to work who can get a man to support her."

This excursion into the general considerations about the work of women is necessary in order to understand the real force of much of the more obscure opposition which exists to women physicians. As has already been said, the profession of medicine cannot be taken up and laid down again, like the lower positions in industrial occupations, or even like the profession of teaching. It must be adopted, if at all, for a life-time. Its ripest fruits cannot be gathered until a ripe age, long past that most suitable for marriage. On this account, and because the women who are most likely to succeed in medicine have often also marked capacities for success in marriage, and because their ability to perform such work as that involved in the practice of medicine, and demanding high organic vigor, tends to be increased after marriage and the possession of children,—for all these reasons, it is felt that the question of women in medicine touches upon the ground not covered by their pre-marital work elsewhere. To the question, "Is it possible for married women to practice medicine?" experience might already be supposed to return an answer. A very considerable number of women now practicing have either married after entering upon practice, or were already married when they began. To what extent either their households or their practice may have suffered by the combination, it is almost impossible to ascertain by the most carefully prepared tables of statistics. It is not yet even certain whether the combination influences the size of the family. As far as can be judged, this remains at the measure most habitual in the families of professional men. Since this paper is intended

to discuss rather theories than facts, it is worth while to outline a typical case, such as is not unfrequently realized:

A healthy girl of eighteen, with an ultimate view to the study of medicine, enters upon a university course, and, at the age of twenty-two, begins medical study. She is ready for practice at twenty-seven, marries at the same time or a year later. Her children are born during the first years of marriage, thus also during the first years of practice, and before this has become exorbitant in its demands. The medical work grows gradually, in about the same proportion as imperative family cares grow lighter. The non-imperative duties—the sewing, cooking, dusting, even visiting—are susceptible of such varied modifications of arrangement as it would be trivial to discuss in these pages. So great is the division of labor in medical work that it is indeed rather the minority of physicians who can consider themselves fortunate in being "overwhelmed" with practice. In respect to the quantity of work performed by women, the same rule may be applied as has been laid down for the admissible proportion for the laboring classes—namely, about one-third of that performed by men of the same grade of success and ability. This fact need not impair the quality of the work. Professional work, which must remain a form of personal or hand labor, cannot be measured by commercial estimates. Beyond a certain point, increase of its quantity tends to impair rather than improve its quality. A mass of work sufficient to involve serious drafts on physical strength must, in medicine as elsewhere, put nearly all women at a disadvantage. But the handling of such large masses is neither necessary nor desirable for any other than pecuniary reasons, and these, both for physicians and for women in general, may be left in the background. To neither is the attainment of a fortune important; for the majority of both it is impossible. This must always remain true of physicians; whether it always remain as true of women as it seems to be at present, depends upon the extent to which they may rise from the bottom of the industrial scale, where they swarm at present, to the top—a question which, however interesting, does not concern our present subject.

The character of medical work, in its external and obvious aspect,—that of examining, watching, and prescribing for sick people,—suggests a degree of adaptability to the exigencies of domestic life which especially commends it to women. This

is one reason why so many choose it, in preference to other occupations. In these reasons for a choice, there lurks, however, a danger, which it is well to distinctly recognize. It is that of overlooking the importance and extent of that part of the physician's work which is performed outside of the consulting room, and away from the patient's bedside. In the mind of the true physician, this bears about the same proportion to the work of practical intercourse with the sick, as for the pianist exists between his hours of preparation and practice and those devoted to public concerts or teaching. It is true that this ideal is rarely maintained; but it exists, and tends to become more and more frequently realized during the progressive development of modern medicine. Now, it is in regard to this ideal that the present generation of women physicians are more liable to be deficient than in regard to the energy or solicitude of their attention to patients. It involves some vigor of mental initiative, and, as has been already noticed, the same women who will respond admirably to training, to direction, or to the pressure of practical necessities are apt to be unexpectedly deficient in this.

Since it is only the gradual progress in mental culture which has, for men, aroused mental initiative in the field of medicine, we may reasonably hope that the same process will have the same result for women also. Already a sufficient number of feminine examples exist to prove that this is quite possible. There is a gradual, but undoubted, increase in the capacity for mental initiative on the part of women in general. Those who study medicine must first share in the general movement; afterward, must become more energetically animated than at present by the intellectual impulses of modern medical thought.

We have already asserted that the idea that culture is a means, not only of training, but of developing force, is not generally accepted among current popular notions. It is, however, fully recognized by authorities, and we need, therefore, spend no time in defending it. Our commentary upon the practice of medicine by women does not profess to be very systematic; it evades argument of rights, statistics, and historical statements, partly because these have already been made in an admirable manner,[1]

[1] See "Study and Practice of Medicine by Women," Dr. J. R. Chadwick, "*International Review*"; "Study of Medicine by Woman," Miss Jex-Blake,

partly because, in our opinion, sufficient data do not yet exist for statistical conclusions. The whole number of women at present practicing medicine is small; thus, only about four hundred can be reckoned in America; only nineteen are registered in Great Britain. From this small number, with the imperfect preparation and surroundings of so many among them, to attempt to draw any inferences as to the theoretical grade of capacity of women for medicine is absurd. Still more absurd to attempt to deduce general conclusions in regard to the mental capacity of women in general—its development, progress, or unimprovability.

Deferring exact researches, our modest intention is simply to attack the floating mass of vague ideas, prejudices, preconceptions, and misconceptions which, in this as in so many other matters, really decides the practical action of the community. Drawing to the close of our brief discussion, we are aware that it will seem to leave several questions not only unsettled, but untouched. If there are so few women who, after all, come forward to study medicine, why is it necessary to disturb oneself, or, in the slightest degree, society, about them?

We answer: It is scarecely forty years since the first woman physician graduated in America; not more than thirty since the first school was opened to them; not more than fifteen since anywhere in the world, they could obtain a university education, and from this, in many parts of the world, they are still excluded. It is not, therefore, surprising that the number of women in medicine is still small.

Further the demands of these few have involved a sacred question—that of justice. It is this little band of women physicians who most conspicuously represent the modern claims of women to share in the general intellectual development of the race. Their demands have not only been refused, but refused

---

"*Fortnightly*," March, 1875; 'Reply to Bischoff on the Admission of Women to the School of Medicine at Zurich," Prof. Herman; Speech of Dr. Henry Bowditch on the Admission of Women to the Massachusetts State Medical Society; Report of Committee on Admission of Women to Harvard Medical School, Chairman Prof. Alex. Agassiz; "The College Story," Prof. Rachel Bodly; "Practice of Medicine by Women in the United States," Drs. E. & A. Pope, and G. L. Call; "Early Practice of Medicine by Women," Prof. Bolton, "*Journal of Science*," January, 1881; "Women in Medicine," "*Michigan Exchange*," Lydia Welch.

with contumely, and themselves treated with the tyrannical contempt which Prussians and Anglo-Saxons habitually reserve for those who urge a claim without the physical force to secure it. This is the first reason why the question, insignificant as regards number, has a real social importance.

The question at issue does not concern the training of scientists or the development of original genius, but the training for a practical work of persons who have demonstrated at least very many of the abilities requisite for it. In this they have put themselves, so far as regards native capacity, on a level with the great majority of the members of the medical profession. It is possible, if not probable, that they have shown themselves less able to dispense with thorough training than their masculine colleagues, less to compensate its deficiencies by self-imposed efforts. No practical inference can be drawn from this other than one which applies to the entire relations of the state to medical education, viz., that every practitioner should be much more closely supervised. Efficiency should be more seriously and more often tested, and the public far better protected than it is at present from the incompetence of both male and female practitioners. It is the height of folly to trust to American ingenuity and quickness to escape the consequences of imperfect knowledge. If it be desirable to establish a rigorous discipline for women physicians, it is no less important to do it for men, so that, practically, the distinction disappears.

In regard to any unpleasant modification of female character, likely to result from medical or other superior education, it cannot be too frequently noted that nothing further is proposed than to make room for the varying types of women which actually exist. Mr. Bagehot declares that an unvarying type of character is characteristic of a barbarian community, because essential to its safety, and is, therefore, enforced by all penalties, even to that of death. Only civilization is flexible enough, and stands on a broad enough basis, to permit internal variations. Surely women may share sufficiently in civilization to be allowed, without social disgrace, to exhibit such variety? The taste for medicine, when profound and genuine, is certainly peculiar enough to establish a decided variation on the most conventional type of women. But many other tastes do the same; and choice remains free.

"The crane, I said, may chatter with the crane.
The dove mate with the dove, but I
An eagle, clang an eagle in my sphere!"

A final reason to be considered in regard to the professional work of women, and its possible continuance after marriage, lies in the re-arrangement of domestic work which has gradually been brought about by the growth of modern industries. As every woman knows, these are built up of tasks which have been withdrawn, one by one, from the control of the household and of its mistress, and have been combined, perfected, amplified to wholesale and often gigantic proportions. The process necessarily liberates the energies of the woman formerly absorbed by these same necessary tasks of preparing food and clothing. The process is still going on, and is constantly simplifying the material mechanism of household existence.

The "invasion of foreign spheres," for which women are so often reproached, is only the natural result of the double pressure of an economic and of a psychological necessity. It is always necessary that human beings be occupied; if driven from one thing they must take up another; if all employment be taken away from them, they must at least pretend to be busy. On the other hand, it is not only obviously desirable that unmarried women find as many avenues for employment as possible, but the possibility of contributing toward the support of a family may decide, for many women, the alternative of marriage or celibacy. The traditional method of effecting such contribution is by means of a dowry; in many cases, it both has been and is the only possible way, since neither the strength not the ability of the woman would enable her to engage in any non-domestic work, and yet do justice to her children and household. Any woman, however, who is possessed of the requisite physical and mental strength should be allowed, at her choice, to contribute the profitable work for which she has been trained, in lieu of a dowry which she may very easily not happen to possess, or in lieu of certain domestic labor which she would otherwise be compelled to perform on account of the narrow income of her husband. Is the sentiment of marriage endangered by the habits of pecuniary self-reliance and independence which should tend to free marriage from its burdensome aspect as a "career," and an indispensable means of livelihood?

All professional work constitutes a form of personal service, and, as such, is much better suited to the prevailing instincts of women than industrial or commercial pursuits. The adaptation on this side is indeed so great as to constitute a danger; for it should never be permitted to obscure—and, in fact, it sometimes does—the cardinal question of capacity. We have said what we think there is for the present to say, in regard to the proof of such capacity in women. Here, in closing, we only wish to refer to its desirableness as a means of stimulating to better efficiency much existing feminine occupation. When room is made so near the top as is the high, difficult, and responsible work of practical medicine, the lower ranks may thin out by promotion. The pitiful overcrowding of the meanest employments by the huddling together of all grades of capacity, from that of the raw Irish girl to that of the reduced gentlewoman, might cease when fair play was once allowed for superior energies.

Have we not had enough of the dictum, "Women cannot do men's work?" In reality, some woman can do the easy work of some men; others can do the more difficult work of more powerful men; finally, there are some men whose work, either for mass or quality, has so far not been equaled by the achievements of any woman. And this is all there is to say about it. It is desirable that every woman remain as inferior to her own husband as may be feasible and convenient; it is for that purpose she marries him, or should do so. But the generalization of this relative inferiority to the comparative capacities of all men and all women, in regard to every work that both undertake, is a most injurious absurdity.

When we shall be rid of the injustice, the unfairness, the monstrous pretensions, and arrogant argument with which the subject of the admission of women to medicine has hitherto been so largely treated; when the mass of women students can obtain the same education and women physicians the same facilities that men do, a sound theoretical conclusion may then be reached, if required. But by that time the practical conclusion will probably have established itself, and people will cease to interest themselves in dissertations on the true theory of *un fait accompli.*

AN ADDRESS DELIVERED AT THE COMMENCEMENT OF THE WOMAN'S MEDICAL COLLEGE, OF THE N. Y. INFIRMARY, MAY 30, 1883.[1]

LADIES OF THE GRADUATING CLASS:—When you first honored me with an invitation, I declined, and for a reason well known to you. I think it extremely difficult to find a theme that shall be interesting at once to medical students, and to these assembled friends, who, though much interested in certain students, can be expected to take but little interest in medicine.

Were the devising of graduation exercises entrusted to me, I should not hesitate to borrow from the ceremonies of the antique Eleusinian mysteries, or from those of the mediæval Rosicrucians. For however much the light of common day and of common-sense may have been let in upon the art of medicine, it still remains a mystery, a sacred mystery, to the uninitiated; that is, to all who have not been submitted to a prescribed discipline.

If, however, I should seek a ceremonial of graduation appropriate, not to the mysterious and difficult nature of the studies you have pursued, but to the arduous personal responsibilities you are about to assume, I might find it in the vigils prescribed to the candidates for knighthood in the Middle Ages. These were required to spend a night in fasting and prayer in a solitary chapel, watching the armor and the scabbarded sword they had not yet been permitted to unsheath. There are so many spiritual resemblances between the duties of an energetic physician and those of a well-armed knight.

> "Bound for the wide world past the river,
> There to put away all wrong,"—

that we should not be far amiss if we imitated these solemn vigils of his initiation. And were we habituated to the accurate

[1] Reprinted from the *Archives of Medicine*, 1883, Editorial Department.

sym lism of a more imaginative age than our own, we should at least take care that the garlands which were offered to you in congratulation, were composed, not of roses, but of thorns. For it is thorns, and not roses, which fitly symbolize the career upon which you have now chosen to enter.

No symbolic or mysterious ritual, however, is likely to be either revived or invented for the graduation of students in medical schools in modern New York. There is, however, a widespread feeling among the Faculty of this School, that the exercises of graduation should be exclusively medical in character; that the examinations should be entirely, or in part, conducted in public, before a medical audience competent to judge of their excellence; that the students should defend their theses; should give evidence of practical conversance with the duties of their profession, by examinations at the bedside of patients. The graduating exercises, in a word, should all be performed by the graduates, and not before them, by some one else. Thus only they acquire real significance and importance. Thus only, I may add, could they be affiliated to the customs of the great European universities, which, in this, as in other matters, must remain our permanent models. These considerations carry so much weight, that I am happy to believe it not impossible that this *may* be the last public commencement, in the popular sense, ever held by our School. In that case, it would be the last occasion on which the graduates and their non-medical friends could consider together some of the non-medical aspects of their professional career.

Now, in this connection, the topic which most frequently suggests itself at our graduation exercises, is that of the sex of the graduates. Indeed, you are liable to be so much and so frequently reminded that you are women physicians, that you are almost liable to forget that you are, first of all, physicians.

As a rule, I have always advised you to reverse this order; to so saturate and permeate your consciousness with the feeling for medicine, that you would entirely forget that public opinion continued to assign you to a special and, on the whole, inferior class of workers in medicine. Still more have I advised you to forget that, in attempting to become physicians at all, you and —far more than you—your predecessors, have in any way braved

public opinion. If it be a new place into which you have entered, it is incumbent upon you to acclimate yourselves as quickly and thoroughly as possible to its atmosphere, and not keep dawdling on the threshold to forever remind yourselves and every one else that you have only just come in. Recently emancipated people are always bores, until they themselves have forgotten all about their emancipation. But those, whose souls are really born free, easily regard the trammels imposed upon them by convention or circumstance as trifling accidents which must necessarily be set aside. They do not dream of glorifying themselves because a barrier has fallen down; if the barrier be an injustice, they know that sooner or later it must fall, and once out of their way they spend no further thought upon it.

There is certainly enough, and far more than enough, in medicine to interest and absorb you, without diverting your attention to questions of your social status, and if you do not find the facts of medicine more interesting than any other facts, you are not fit to be physicians. There are, however, occasions on which it is proper to consider the fact that you still constitute, to a considerable extent, a class. You have, therefore, a certain class of interests, and it is important that you should neither overlook these, nor belittle their real importance. For if medicine, or rather, biological studies in health and disease, be to us the most interesting of all subjects, we must admit that after this, the overthrow of social prejudices, tyrannies, and monopolies is, perhaps, the next most interesting theme that could engage the attention of any one. And of all monopolies, what has ever been more odious than that which has restricted to one half of the human race the advantages of education and the facilities of increased life which that confers, while the other half of humanity has been forcibly excluded from both?

It is true that this monopoly, like all other class monopolies which ever existed, could be defended at first as a simple expression of a natural order of things, and afterward by all the force of the association of ideas which this original order engendered. Yet there never was a time when the monopoly was not self-contradictory and injurious. There was no business reason why women should not have been educated in ancient Athens, for there, education was only designed for refining social intercourse. But, as every one knows, the more respectable and

high-toned the woman, the less was she allowed to be taught. There was no family reason why the celibate nuns of the Middle Ages should not have shared in the early movement toward learning which began in the monasteries; but it is certain that they were not so allowed. There is no economic reason why in modern England, with its thronged population of unmarried women dependent on their own exertions, the slightest opposition should have been offered to the opening of a new profession to woman as a means of livelihood. But in no part of the civilized world, not even in America, has opposition to women students and practitioners of medicine, been so bitter, so brutal, so densely organized, so versatile in its resources, so multiple in its hypocrisy, as in England.

The more we reflect upon this opposition, the more incomprehensible does it appear. Let it be admitted that, for one reason or another, the mass of women had shown, or rather had appeared to have shown, indifference to learning and to the higher forms of work. Should we not have supposed that every class in the community would have hailed with the liveliest satisfaction the first manifestation of such interest on the part of women? Granted that facts seem to justify, at least a provisional scepticism, in regard to the ability of women to profit by a professional education in abstruse subjects, why should any one have hesitated to offer the fullest opportunities for the development of their powers and the decision of their individual capacity? The mass of argument, sarcasm, ridicule, invective, and downright calumny which has been poured out upon the heads of the women who, for the last thirty years, have been trying to study medicine, can only be explained by the constant tendency of all monopolies to strengthen themselves by injustice, as soon as they feel that their exclusive privileges are menaced. The argument most frequently brought to the front is, that the presence of women must lower the prestige of any institution to which they were admitted as co-workers or fellow-students with men. When the London University was debating the question of opening its degrees to women, the medical journals received many letters from former graduates of its medical school, solemnly protesting that such an admission would be a violation of their vested rights,—since it must necessarily lower the value of their diplomas long ago earned and paid for. The same considera-

tions have dominated the action of the Harvard Medical School in this country. In this city a few weeks ago, when a young lady physician had successfully passed a competitive examination for position as interne in one of our hospitals, one of the examiners remarked that he should be opposed to her admission for this reason: there would be no difficulty so long as she held the junior position; but when, in ordinary course of promotion, she should advance to the higher grades, it was to be feared that new candidates of value would not present themselves for the ensuing vacancy, when they heard that, if successful, they must serve under a woman as a superior officer. Now it happened that at the last examination which had been held at this hospital, the results were so unsatisfactory that all the candidates were rejected. Had this happened after the nomination of the lady in question, —for she was appointed,—how easy it would have been to infer that it was her presence in the hospital which had deterred suitable candidates from presenting themselves!

There have been but three other occasions on which women have attempted to compete for positions in New York hospitals. On the first, the candidate passed a successful examination, and was admitted without further ado. On the second occasion, when a vacancy was open at Charity Hospital, the woman candidate was acknowledged to have beaten her competitors, but was then refused the place for which she had worked so hard. On the third occasion, a young lady attempted to come up for an examination which was announced for vacancies in the staff of assistants at the female insane asylum at Blackwell's Island. The Commissioners of Charity promised that she should be considered eligible; but one of the medical examiners deliberately misinformed her as to the date of the examination, so that she could not present herself. The resident superintendent, temporarily in charge, further declared that he should in any case decline to be governed by the results of a competitive examination; that he should appoint whom he chose, and he certainly should not choose a woman.

Now Dr. Tuke, the famous English alienist, who had been studying the organization of insane asylums all over the United States, has expressly declared that the best conditions were invariably found where the female patients of the asylum were under the charge of a resident female physician. Similar testi-

mony comes from every asylum where women physicians have been installed. Nowhere is it more desirable that they should obtain a footing than in the vast bedlams of our city almshouses for the pauper insane. But because a handful of persons in charge happen to dislike what they consider an innovation, all attempts to secure competent female assistants on the islands have so far failed.

Hospitals, as well as universities, belong, of right, to the communities which support them. Nothing can be more absurd than the asumption, almost universally made, that either the trustees who administer such public institutions or, in the case of hospitals, the physicians who visit in them, have the right to monopolize their privileges to the exclusion of any duly qualified citizens. In regard to universities and professional schools, it is well known that this assumption is far from being tacit. Applications for admission have been made by women over and over again, and refused as calmly as if these public institutions were pieces of private property, upon which intrusion was an impertinence. In regard to hospitals, the question has been much less sharply defined, because as yet few candidates have presented themselves. Few women have had the courage to undergo a long and expensive preparation for an examination, to which, after all, they might at least be pronounced ineligible. No woman has even ventured to apply for a position in the Woman's Hospital, where, if anywhere, it might be presumed that a woman physician should be entitled to a place. It has been stated, I know not on what authority, that in this particular hospital the Board of Lady Managers would peremptorily oppose the admission of women internes, even if they had conquered their place by competitive examination, and had overcome the prejudices of the medical staff. It is very certain that not a member of this Board has ever taken the slightest step toward securing the services of a physician of their own sex for the women under their care, nor toward throwing open the advantages of this hospital to the women physicians who might worthily profit by them.

This practical monopoly of the vast clinical opportunities contained in the hospitals, dispensaries, and city institutions of New York, can not, however, be made a matter of serious complaint until competent women candidates shall have come forward in

greater numbers and with more determination to demand their share of this public property. When the demand is once made, it cannot but make itself heard. When at least half of the hospital population are women, and sometimes a third are children; when female nurses are being trained in large and increasing numbers within the hospital wards, it is absurd to allege that from motives of either delicacy or convenience, female physicians must be excluded. If, in certain hospitals, the existing arrangements are such that a woman interne could not perform all the duties, then the existing arrangements should be modified whenever a woman candidate shall have demonstrated her intrinsic fitness for the place at a competitive examination. There is no good reason why, in such a case, the female wards of the hospital should not be assigned to the woman interne, the male wards to her masculine coadjutor.

The appointment of women on the staff of visiting physicians to a general hospital is a question that has not yet come up.

This subject of hospital appointments well illustrates the close solidarity of interests which exists between women physicians. It will not do for you to forget this, and to imagine that when you have once secured your several diplomas all your class work ends. It will not do to imagine that devotion to your own individual interests and advancement will suffice to secure even that. You must combine to remove the difficulties which stand in your way as a class, and to which the fortunes of any individual among you are always liable to succumb. The habitual exclusion of women from fit opportunities for preparation or exertion, engenders an habitually low tone of confidence in their abilities, which constantly interferes to prevent any given woman from demonstrating her abilities. We have not yet reached the time when it will be considered as natural for a family to employ a woman physician as a man; or where the profession of medicine will be evenly distributed between men and women as is now the profession of teaching. To bring about this state of things requires much effort, individual and collective, persistent, patient, far-sighted, indomitable. The problem involves questions of rights, but is by no means only a question of rights. An inequality must be rectified, and in the teeth of much opposition; but the most delicate part of the task consists in actually

raising to an equality the class which hitherto has been really inferior.

You may contribute to this great work in two ways. In the first place, you should be continually exerting yourselves to increase the educational advantages of the school of which you are alumnæ, and also to extend the opportunities for undergraduate education elsewhere. It is strange how little our graduates have hitherto exerted themselves in this respect. The seventy-five whom we have by this time sent forth from among us, could, if solidly united in purpose, immensely increase the educational facilities of those who come after them. But it is a short-sighted policy to imagine that the affairs of the school no longer concern you because you will never be obliged to re-enter its undergraduate course. On the contrary, who is to look after them if not you? Why should outsiders, from motives of pure philanthropy, busy themselves with collecting support for institutions and enterprises which should by this time be managed by the collective efforts of the college graduates? There is another kind of educational effort which it is most important for you to make: I mean the continued education of yourselves. It is a commonplace of commencement addresses to remind you that your education is only just begun; that you must continue to study and improve, and so on. I am not speaking however in this general sense, but with reference to a certain peculiarity whose importance has probably not yet impressed you. This is, the remarkable contrast apt to be shown between the energy which women will manifest in obeying authoritative orders for study, and the lack of energy they show in independent initiative. Experience in medical, as in other tuition, has abundantly proved that in every class there is always a fair proportion perfectly capable of learning all that can be taught them. When such students are found insufficiently prepared on any subject, we may justly lay the blame to some defect in the method of teaching. And at present, the method in vogue of teaching the medical sciences is so defective that it is not surprising so many students remain so far below their real capacity of attainment. But under whatever guidance a student is instructed, there comes a time in which he must become his own guide; in which further knowledge must be obtained in obedience to his own consciousness of its interest and necessity; in which further dis-

cipline must be self-imposed. And it is precisely here that women students are apt to fail, to stand still, to abandon all definite intellectual purpose, and begin to drift like rudderless ships. When we consider the often enormous efforts and sacrifices made by women to secure opportunities for study and to work their way toward a diploma, it is nothing less than astounding to notice the intellectual apathy into which so many sink, as soon as the coveted parchment is secured.

Comment on this circumstance may perhaps be deemed inappropriate on this occasion, and a discouraging endorsement of a widespread reproach that has long enough been made to women:

"Yea," said Cyril: "they learn the old things as well as we. But when did women ever yet invent?"

I have, however, a word to add in at least partial explanation. Lack of intellectual initiative is by no means confined to women; it is, in fact, the average condition of the human race. Few, and far between, are the minds sufficiently vitalized, self-reliant, and self-poised, to be able to disengage themselves from hand-to-mouth, every-day necessities and preoccupations, and to pursue an ideal inquiry for its own sake, or for the solitary pleasure of rounding off and completing their stock of knowledge on any given subject. The great mass of intellectual work that is done in the world, is still done in obedience to order; more remote, less direct than that which lays down the curriculum for undergraduate studies, but still an order which emanates from some superior mind, or from the collective intellectual force of the community. This work is being incessantly stimulated by a complex machinery of societies, publications, prizes, places, reputations, innumerable rewards of most varying character, but all consciously or unconsciously directed toward fostering the mental activity of those who would not work without them.

Now, to the extent to which women continue to isolate themselves, or to submit to enforced isolation from this vast current of intellectual life, it is inevitable that their own must become apathetic. All impulse to energy finally comes from without, as all life depends upon the sun. Before, therefore, much stress can be laid on the reproach of lack of initiative in women, it behooves us to consider whether the position in which they now are is one in which mental initiative has ever been developed on a large scale among men. Their position is colonial; and every

one knows the singular combination of mental inactivity with intense practical energy, which peculiarly characterizes colonial life.

The disingenuous hostility to women physicians, which has marked every step in our thirty years' struggle,—we may justly call it a Thirty Years' War,—has much abated in regard to the elementary question, whether women should receive legal authority to attend such sick persons as chose to consult them. Fortunately for us, the habit of consulting with reputable women practitioners has been established, some time before the present concession to consultations with homœopaths could have robbed consultations with women of all significance. But the effort to exclude women from the full privileges of the profession still continues; is manifested in such struggles as that which convulsed the Massachusetts Medical Society at its centennial; in such resolutions as that which excluded women from the International Congress at London; in the annual debates over their admissions to the British Medical Association; and in the discussions, of various degrees of acrimony, which are excited by the application of a woman candidate to any medical society where a woman has not yet been admitted.

To overcome all this opposition it is necessary not only to make persistent application to these same societies, but to engage resolutely, and without the aid of their stimulus, in the same work in which they are engaged. Our English colleague, Dr. Frances Hoggan, has always been excluded from the Pathological Society of London. But the original work in histological investigation that she has pursued with her husband in their laboratory at home, has received deserved recognition in the leading journals of France, Germany, and even England. It is the old story of the bricks, to be made without straw; of the shield to be hammered by the Antwerp artificer, without tools. The task is difficult, extremely difficult, but it is by no means impossible. The important thing is to recognize the necessity for constant definite mental work in definite directions; and the conditions under which this can be performed. This may not always seem to have any bearing on the practical work you may be at the time engaged in. But you may be very sure that if you attempt nothing but what seems at the time absolutely necessary, you will always remain wofully below the measure of the needful. In

intellectual life it is not altogether a paradox to say, "Give us the luxuries, and we will dispense with the necessaries."

Evidence of a free, self-sustained, self-reliant intellectual activity is justly demanded as proof that a physician is capable of exercising the independent judgment which is absolutely necessary for the handling of the simplest case of disease. You cannot treat the sick by means of folios of precepts, the most precise and accurate that were ever devised. And to be able to modify the precepts which you have been taught as a basis for your self-instruction, your minds must have been trained to inquiry, to independent pursuit of knowledge, to the grouping of facts, to the summing of evidence, to the original observation and suggestion which a free mind pursues as its natural and inevitable occupation. Do not, therefore, continue to justify the old assertion that the only free choice a woman ever really cares to exercise is that of choosing her own master. If you cannot learn to act without masters, you evidently will never become the real equals of those who do.

What a number of distinct and different views of things you must therefore hold steadily before you! You must, on the one hand, forget that any social prejudices stand in your way as physicians: but on the other hand you must remember that, in virtue of these, you continue to have certain class interests, which can not, with either justice or safety, be ignored. You must remember all that you have been taught; and yet you must soon cease to think of what you have been taught in comparison with what you must freshly learn. At certain times you must be able to sink all immediate practical considerations in the interest of pure ideas. Yet, to the pursuit of these, you must bring a tenacious, practical energy, such as can scarcely be acquired except in conflict with practical emergencies. There is not a detail of your career, theoretical or practical, individual or social, that will not require the highest possible development of your powers of will. This is, indeed, the sovereign power of human nature, without which bright perceptions, good intentions, quick intuitions, flash only for a moment to vanish in darkness. The beautiful paraphrase of the English poet does not inaptly render the Bible parable:

"Oh, well for him whose will is strong!
He suffers, but he will not suffer long;

He suffers, but he cannot suffer wrong.
For him nor moves the loud world's random mock,
  Nor all calamity's hugest waves confound,
Who seems a promontory of rock,
  That, compassed round with turbulent sound,
In middle ocean meets the surging shock,
  Tempest-buffeted, citadel-crowned."

OPENING LECTURE ON DISEASES OF CHILDREN, AT THE POST-GRADUATE MEDICAL SCHOOL, NEW YORK.[1]

DEFINITION.—At the outset of this course, which I am to have the honor of delivering to you, I deem it appropriate to define the circumstances which serve to specialize diseases of children in the wide field of human pathology.

Setting aside certain superficial practical considerations, we may come at once to the consideration which constitutes the basis of this specialization. It is the fact that the child represents an organism in a state of continuously progressive development. The details of this development may be classified into four groups, namely:—

(A.) Those concerning the adaptation of the organism to a succession of different media, and to a succession of changes in its relations to surrounding objects.

(B.) Those concerned in an increase of bulk.

(C.) In an elaboration of structure.

(D.) In an evolution of function.

Transition to Different Media.—The successive adaptations are necessitated by a series of transitions:—

(1.) From the placental to pulmonary respiration.

(2.) From vascular or imbibition nutrition to nutrition through processes of ingestion and digestion.

(3.) From the recumbent to the erect position.

(4.) From passive nervous and reflex muscular action to voluntary movement, mental action, and locomotion.

Rapidity of Change.—The rapidity and intensity of the changes through which the infantile organism passes both increase in proportion as we approach the birth point, and diminish

[1] Reprinted from the *Boston Medical and Surgical Journal*, 1883.

as we recede from it. Thus the first hour, first day, first month, first year, may each be considered as distinct periods, covering changes of such magnitude as are effected in no other hour, day, month or year of existence. For in the first hour of the birth of the child, pulmonary respiration is established, involving a complete alteration of the intra-cardiac circulation together with extensive changes in that of the liver and entire abdomen. The arterial blood pressure becomes superior to the venous: the oxygenation of the blood passes from a minimum to a maximum; the functions of the heat-regulating apparatus, hitherto rendered unnecessary by a medium of constant temperature, are now initiated. During the first day the functions of the alimentary canal are established; the prehensile capacity of the mouth; primary digestion in the stomach and intestines; absorption and excretion. Extensive desquamation begins over the entire tegumentary surface,—in the alimentary canal of epithelium, on the skin of epidermis. This desquamation continues during several days of the first month, and accompanies the first effort of secreting activity on the part of the buccal, peptic, and intestinal glands on the inner, and of the sweat glands on the outer, tegument. The salivary and pancreatic glands do not elaborate effective secretions until after the close of the first month. During its first half, however, occurs the fall of the cord and healing of the umbilical wound; the adjustment of equilibrium in the hepatic circulation; the emptying the middle ear of accumulated epithelium, by successive efforts of deglutition; the habitual working of the heat-regulating apparatus; the development of the psycho-motor centres in the brain. The latter, according to the experiments of Soltman, become for the first time responsive to faradic excitation on the tenth day after birth in puppies. It is probable with the much slower rate of development of the human young that in them these centres become excitable much later.

During the first year not only these but all the ganglionic centres in the cerebro-spinal axis become elaborated in structure. The special senses are trained, and the centres in which their nerves terminate become habituated to decompose the impressions these transmit to them. The power of voluntary movement is acquired; the growth of muscles, of the bony skeleton, and of the teeth in their alveolar cavities constitute prominent

phenomena of the physiological life. The heart doubles in size (from a capacity of 20 cc. at birth to 40 cc.); the lungs treble themselves. (Beneke.)

The period of the child's life embraced between one and three years is marked by the completed eruption of the teeth; by change in diet, and hence in process of digestion; and by the further evolution of nerve centres connected with the acquisition of speech, the awakening of intelligence, and the attained ability for station and locomotion. The latter again implies continued progress in the development of the musculo-osseous system.

Between the age of three and seven years begin the social relations of the child; from seven to fourteen, and especially in the latter years of this period, the growth of the body, and particularly of some of its organs, becomes again extremely active, and preparation begins for the development and functions of the reproductive organs.

Correlative with each period thus demarkated, we find a preponderating liability to some special morbid condition. The correlation is rarely exclusive; few diseases are absolutely limited to any one period of life; but there are preponderances indicating an approximate limitation. An absolute limitation exists in regard to the morbid conditions associated with congenital malformations; and also in regard to the group of diseases known as those of the new born. The former are necessarily present at birth, and discoverable in the first hour of existence. The latter arise from possible accidents of parturition, or during the successive movements of transition effected during the first hour, day, or month. Thus, in the first hour we may encounter the formidable accidents of asphyxia, and arachnoid hæmorrhage, and the far lesser danger of subperiosteal cranial hæmorrhage constituting cephalatoma,—all dependent on abnormal pressure received by the child during the process of parturition. It is at the same moment that the fatal apnœa of diaphragmatic hernia is also, if ever, observed.

During the first day, or even the first two or three days, secondary morbid processes consecutive to an asphyxia or apoplexy of parturition are liable to appear. The infant whose respiration has been established after laborious efforts may develop pneumonia in consequence of intrauterine aspiration of foreign matters into the air passages. This is a "Schluck-

Pneumonie," analogous to that caused by section of the vagi nerves, and paralysis of the glottis. A portion of the lungs may remain unexpanded in fœtal atelectasis, or subsequent portions may collapse from weakness of the inspiratory efforts. Such atelectasis is a not unfrequent accompaniment of meningeal hæmorrhage. The latter again, initiated during the birth process, may continue to extend, not proving fatal till several days after birth. The cerebral hæmorrhage may of itself occasion no characteristic symptoms, but by extension along the medulla, or into the spinal membranes, may determine tetaniform convulsions.

Paralyses rarely or never arise from arachnoid hæmorrhage, but often coincide with them as a result of the same cause. The cadaver that I here show to you is that of a child who was found on the first day to be paralyzed in the right arm, and on the second day began to exhibit symptoms of arachnoid hæmorrhage over the left cerebral hemisphere. But the paralysis was due to compression of the right brachial plexus.

Meningeal hæmorrhage is not always the result of a parturient traumatism. It may be the expression of a puerperal infection, its symptoms so dominate the rest, that they are overlooked. Thus, the infective process tends, in the new born as elsewhere, to cause fever; but this tendency may be so checked by the hæmorrhage that the temperature falls even below normal.

Instead of paralysis from compression of nerves, the child may suffer fracture in one or more limbs. The baby I now show you was born with fracture of the right humerus, and also of the right femur. The mother was attended by a German midwife who summoned a physician, and both together applied some kind of splint to each injured limb. But the adjustment was so imperfect, that to-day, two weeks later, the fragments of the humerus are still freely movable on each other, and the fragments of the femur have united at an obtuse angle.

A prominent accident of the first day or days of infantile life is umbilical hæmorrhage. Certain English authors speak of this accident as one easily controlled. This is, however, only the case when it is dependent on a local process; but when, as is much more frequent, the hæmorrhage is the expression of constitutional disease, it is usually fatal. Under this microscope are slides representing sections from various tissues in such a case,

where the hæmorrhage began two hours after birth, repeated itself frequently, and proved fatal in ten hours. It depended on a generalized fatty degeneration of the blood-vessels

This fatty degeneration may begin in fœtal life, and be associated with acute fatty atrophy of the liver (Hecker and Buhl); or it may occur as an element of the complex process of the puerperal infection of the new born. This disease again may begin before birth, and even run its entire course in utero, the child being born dead with lesions of general peritonitis. After birth the same pyæmic process may be, now the cause, now the result of inflammations of the navel, or of the umbilical vessels.

Pyæmia and the acute fatty atrophy or Buhl's disease are both accompanied by severe icterus; a milder form of jaundice is extremely common during the first week of life, the well-known icterus neonatorum. The semi-physiological character of this symptom invests it with more interest than it could derive from its clinical importance. By some authorities it is considered only apparent, and to be dependent on the flushing of the skin during the process of the establishment of respiration and the desquamation of the cuticle. By others again it is associated with the changes in the hepatic circulation. The tension is said to be so lowered in the hepatic veins, after closure of the umbilical vein, that bile passes freely into them from the biliary capillaries. Finally, the icterus has been attributed to destruction of the blood corpuscles, and this hypothesis again divides itself between two theories: according to the first, the destruction takes place within the blood-vessels at large, and the color of the skin depends on the accumulation of hæmatoidin, chemically identical with bilirubin. A much more tenable theory is based on the fact, recently demonstrated, that icterus is associated with an excess of the denutrition,—of the loss of weight,—normal to the first days of life, before the digestive processes are able to compensate the processes of waste. The blood corpuscles are attacked, in common with other albuminoid substances are destroyed in the liver, and their coloring matter occasions the formation of an excess of bile.

On escaping from the sphere of parturient accidents or diseases, and the time at which the various malformations may be properly studied, the child passes during the second half of the first month into a second region of liability, where disease de-

pends on the imperfect function of organs. After the establishment of pulmonary respiration, the initiation of extra-uterine nutrition involves a series of processes, whose equilibrium is very easily overthrown. During the period of normal insufficiency of the digestion the metamorphosis of albuminoid substances—of which part is derived from the stored albumen of the tissues—is also incomplete. Hence, together with the formation of a large amount of urea,—also occurs the formation of an excess of uric acid. This may accumulate in the canaliculi of the kidney, constituting the renal infarctus of the new born; a lesion constantly discovered after death from malnutrition during the first weeks of life. [1]

A still greater degree of nutritive failure involves encroachment upon the fixed albumen of the tissues, and the child visibly emaciates. The imperfect digestion is indicated by acid fermentations of food, with consequent colic, vomiting, diarrhœa; or, as a result of the continued irritation of the digestive mucosa, catarrhal inflammation sets in. The growth of thrush (oidium albicans) is facilitated by the desquamation of epithelium normal to the period. The ultimate result of these gastro-intestinal disorders, if unchecked, is a state of profound and pitiful denutrition, variously known as marasmus, atrophy, or athrepsia.

The tendency to gastro-intestinal disease persists throughout the first year. A similar liability exists towards disease of the respiratory apparatus, and for the same reason in both, namely, the novelty of function. This contrasts with the immunity from disease of such organs as the heart, liver, and kidneys, whose functions have been exercised during several months of fœtal life. These organs seem to become liable to disease as they wear out from long use, not as they enter upon their functions.

During the first year two constitutional diseases first show themselves, hereditary syphilis in the first quarter, rachitis in the second quarter or half. Both depend on perversions of nutrition, on the one hand, through the influence of a specific virus; on the other, through digestive failure and anæmia.

The three remaining periods of morbid liability can only be defined approximately. Thus, from the age of one to three, there is a greater tendency to certain forms of nervous disease.

[1] Virchow supposed that the renal infarctus is normal to the new born. But this hypothesis has been rendered improbable.

The maximum liability to meningitis is at two years. (Rilliet and Barthez.) Convulsions are associated both with processes of dentition (through the medium of a co-existing rachitis) and with digestive disorders, the latter frequent from the change of diet at weaning. The evolution of function in the nerve centres at this period of life constitutes a predisposition to nervous disorder. Scrofulous malnutrition, tuberculosis may be assigned to this same period, because, on the whole, it is then that they most frequently manifest themselves for the first time.

The fifth period, embracing the four years from three to seven, is especially characterized by the liability to infectious diseases, as diphtheria, the eruptive fevers, pertussis. This tendency is evidently associated with and partly caused by the widening social relations of the child, by which he is much more easily and frequently brought into contact with sources of infection.

Finally, the sixth period, extending from seven to fourteen, continues the morbid tendencies of the last two; shows a greater tendency to diseases of the osseous system; exhibits more distinctly the marks of constitutional anæmia, and develops the liabilities to rheumatism, and to certain neuroses, for example, chorea, epilepsy, hysteria.

As we approach the birth point in the survey of the child's history, we are made aware of the persistent influence of many conditions which have dominated fœtal and embryonic life. The process of cleavage, the process of curvature, the principle of inequality in the growth of parts, and that of mutual limitations of growth, govern, taken together, the course of embryonic evolution. They constitute a set of mechanical conditions which, in continuation with the one vital process of continuous growth, suffice to produce the marvelous results of that evolution. The mechanical influence, principally of pressure, is exerted in the first place by the membrane investing the embryonic mass, or, in other words, by the external medium; in the second place by the parts of the organism reciprocally upon one another. Throughout childhood this special susceptibility to mechanical influences persists, and, as in embryonic life, these are exerted first, by the external medium upon the growing organism, second, by the parts of that organism reciprocally upon each other. This is a fundamental fact which offers the key to many

of the peculiarities of infantile pathology, as we propose to show in detail.

The fertile results which may be obtained from the simple influence of continuous pressure exerted upon masses continually increasing in size is remarkably shown in the process of cleavage. This is not primary or self-determined, but is brought about, first, by the pressure of the investing membrane upon the protoplasmic masses, second, by the retractile force of these masses themselves. The segmentation of the ovum—the first step in its development after fecundation—offers the first example of this process. This diagram, enlarged from Kölliker, exhibits the segmentation of the ovum of an ascaris. The second example is found in the cleavage of the blastoderm into three layers: the ectoderm, entoderm, and mesoderm. It is shown in this diagram. The formation of the cleft for the spinal column, of those for the pleuro-peritoneal cavity and pharynx, of the bronchial clefts, the segmentation of the provertebræ, and finally the segmentation of the limbs, are all important illustrations of the process of cleavage determined in each case by the double mechanism above described.

After birth no cleavage process ever occurs, but we encounter several pathological conditions dependent on errors of cleavage, on its excess, more rarely on its deficiency, most often on the persistence of clefts, which, in the normal course of development, should have become closed. A complete cleavage of the ovum, previous to the differentiation of its parts, results in a twin pregnancy. It implies an excess of formative material, but may be considered as the first step towards a monstrosity. (Ahlfeld.) Partial cleavage of the caudal or cephalic extremity, or both, results in the development of different kinds of double monsters. Excess of cleavage at the extremity of the limb buds causes supernumerary fingers or toes. I here show you a child presenting such a deformity. The process of cleavage, continuing after the different fingers had been differentiated, has split up the thumb segment into two unequal parts. In the larger or internal segment the two phalanges continue in a straight line with the metacarpal bone. The smaller external segment stands out at an angle with the metacarpal bone, being apparently articulated with a facet on its distal and outer extremity.

Failure of cleavage determines various degrees of apparent

fusion of parts, a deformity again principally observed in the limbs. Web fingers or toes constitute the lightest grade of this deformity. The fingers are well formed, but connected by a fleshy membrane, not difficult to divide, but whose parts show an almost incoercible tendency to grow together again. The fingers may, however, be fused into a solid mass; the two lower extremities may remain united in a species of tapering tail, and constituting the viable monster known as the siren.

The most numerous malformations, however, connected with the process of cleavage are those which result from a failure to close of a cleft destined only for temporary existence. Thus persistence of an extensive portion of the dorsal cleft determines a hemicephalus, of a more limited portion, with protrusion of the spinal membranes, a spina bifida. Of the latter malformation I here exhibit to you a living specimen. The hernial tumor is situated, as you will notice, in the seat of election, the lumbar region, the vertebral arches being here entirely deficient. The greater frequency of spina bifida in this region is explained by the fact that closure of the dorsal cleft is effected from above downwards, as is shown in this diagram. Hence the lumbar region remains open for the longest time.

I defer more detailed study of this interesting case to another occasion. I merely point out here that the hemispheric tumor, as large as half of a large orange, is deprived of skin, and its thin, membranous surface superficially ulcerated; that the child, which is seven months old, though enjoying fair health, is much emaciated, and, for a month or two, has exhibited the symptoms of a chronic internal hydrocephalus; finally, that the lower extremities are congenitally paralyzed, and both feet are in marked talipes calcaneus. It is probable, therefore, that a dropsy of the central canal of the cord exists, which has gradually extended to the ventricles of the brain, of which the central canal is normally the continuation.

On the anterior surface of the body malformations are frequently encountered, which result from failure of union of the lateral segments of the body. Hare lip and cleft palate (the deformity is shown in this baby) are caused by the failure of fusion between the superior maxillary processes, with the frontal or intermaxillary process. The relative position of these parts at the eighth week of fœtal life is shown in this wax model. The

deformity caused by the interruption, between the ninth and tenth week, of the normal process of fusion, constitutes ninety-nine per cent. of all cases of malformation of the face.

Fissue of the sternum, or, from partial failure to close of the abdominal plates, umbilical hernia, and vesical ectopia, may be mentioned as further illustrations of malformations dependent on abnormal persistence of cleavage. On the lateral walls of the fœtus partial persistence of the bronchial clefts leads to certain congenital cysts of the neck; while within the pleuro-peritoneal cavity, though the abdominal plates be closed, failure in the development of one lateral half of the diaphragm necessitates the fatal accident of diaphragmatic hernia.

"It is no explanation," observes Ahlfeld, of these malformations, "to say that they result from an arrest of development. The question is, What has caused such arrest?" and the author assigns five principal causes: first, an insufficiency of formative material; second, pressure from without of too tense amniotic membranes, together with, possibly, deficiency of amniotic liquor; third, pressure from within of dropsical effusions; fourth, interposition, or even adhesion of amniotic folds; fifth, prolapse of some part between the segments destined for fusion.

The process of curvature is as important as that of cleavage in embryonic evolution. To understand its causation and consequences, His advises the student to take a cylinder of wax and slowly press down one extremity towards the other, in the long axis of the cylinder. This will cause a bulging out on each side of the compressed extremity, and the development of a transverse cleft running across it, and connecting the bulging points. The cleft becomes more apparent when a hollow cylinder of India rubber is used, instead of a solid cylinder of wax, and the extremity is curved forward at the same time that it is compressed. It then not inaptly represents the buccal cleft, which develops across the cephalic extremity of the embryo, at the moment that this undergoes the so-called "cephalic curvature" forwards. This curve is shown on this wax model, and also on this diagram enlarged from His. Not only the buccal cleft and cavity, but the lense cavities are formed by this same mechanism; the oculo-nasal cleft is deepened, the forehead acquires prominence, and lateral folds or ridges are thrown up in the neck, between which the bronchial clefts are destined to appear.

The cephalic curvature is observed only in the embryos of mammalia, birds, reptiles; and these alone exhibit the development of the amniotic fold over the cephalic extremity, known as the amniotic hood. His derives the first of these nearly contemporaneous processes from the second, showing that the pressure of the amniotic fold upon the constantly growing head of the embryo necessitates the forward curvature of the latter. The mechanical influence of pressure, combined with the single vital process of continuous growth, thus again suffices to determine most complex results.

The same influence of external pressure is continued after birth, throughout infancy and childhood, and, indeed, until all portions of the organism have become solid enough to resist it. The physiological curves of the spinal column are developed by the weight of the head and shoulders when the child begins to assume a sitting and upright position. When the weight to be supported increases out of proportion to the process of consolidation in the vertebræ, and it become unequally distributed, the pathological curves of scoliosis are formed. The fact, recently established by Beneke, of the great increase in the volume of the heart and lungs about the period of puberty is probably correlative with the special morbid liability to scoliosis noted for this same period. The greater size of the right lung, which has often been alleged as at least one cause of the curvature of the cervico-dorsal spine to the right, may more plausibly be considered so, when it is known that the lungs at early maturity have attained to twenty times their volume at birth. The heart, whose inclination towards the left should balance the excess of weight of the right lung, only increases twelve or thirteen times its original size. But it has been further noted by Beneke, that, in anæmic and phthisical persons, the development of the heart which should take a sudden leap forward during the year of the establishment of puberty remains insufficient, and does not attain the size which is needed as a make-weight to the lung.[1] It is precisely such persons as are most liable to the scoliosis of adolescence. They are also liable to excess of growth of the osseous framework of the body, out of proportion to the vigor of its viscera;

[1] The influence of such defect in lowering the force of the pulmonary circulation, facilitating caseation of inflammatory products, is of course of even greater importance.

hence, again, to an excess of weight to be supported by an abnormally soft spinal column.

The more extensive softening, especially of the intervertebral disks, caused by rachitis, occurs at an earlier period, while weight is evenly distributed, and the influence of sitting postures, right arm exercise, etc., have not yet been experienced. The curve, then, instead of being lateral and partial, is general, as in cyphosis. A case of this kind in an extremely rachitical boy of five years old was sent to my clinic as paralyzed. He indeed could not walk nor hold his trunk erect; but upon being supported in a plaster jacket he was able to do both, and under the use of cod-liver oil, iron, and lime, entirely recovered.

Depression of the ribs, of the cranial bones (cranio-tabes), curvature of the femurs and tibiæ, are all well known results of the influence of pressure steadily exerted upon the softened bones of rachitical children. The lower limbs frequently exhibit the characteristic curvatures before they have ever borne the weight of the body. The deformity seems, then, to be attributable to the inability of the extensor muscles to correct the attitudes of flexion first assumed by the limbs.

Another interesting illustration of the influence of weight, or the pressure exercised by it, is offered by the deformities consequent upon infantile spinal paralysis. A certain number of authors have explained these deformities by the persistent terms of non-paralyzed muscles whose antagonists had been paralyzed. In reality, however, the deformity is due to the influence of a weight which tends to press the limb in a certain direction, from which the paralyzed muscles are unable to withdraw it. Rachitic paresis of muscles or softness of ligaments have the same result. The weight of the body pressing in the direction of the long axis of the femur from above downward, and from without inward, comes against the internal lateral ligament of the knee-joint, and being insufficiently resisted by the contraction of muscles which should maintain the leg in a straight line with the thigh, constantly exaggerates the angle existing between these two segments until genu valgum results. Valgus of the foot is formed when similar pressure is exerted at the internal lateral ligament of the ankle-joint; flat foot when the arch of the foot is pressed down by a dead weight inadequately taken up by the elastic force of plantar muscles and ligaments.

The effects of pressure are most striking in relation to an undeveloped organism, because the part which has been deviated in a vicious direction continues to grow in that. The effect of the deviation, therefore, not only persists, but continually tends to increase. But all living parts, even ceasing to increase, are subjected to a constant molecular change by the movements of nutrition, and these movements are liable to be deviated in an increasingly intense degree by the influence of even an unvarying pressure even in an adult or slowly growing part. Hence therapeutic influences are of much value. Pressure and position may be utilized to promote lymphatic absorption; to change currents of circulation; to determine local atrophy. Probably the value of such mechanical agencies is as yet only imperfectly appreciated. Their power is best to be learned by studying their influence upon embryonic evolution and in the physiology and pathology of childhood.

The third fact of embryonic development which we find still potent in childhood is the inequality in the rate of growth of different parts of the organism. This unequal growth is, during embryonic life, an important factor in the formation of the folds, clefts, and curves we have just been considering. After birth, the same inequality persisting, determines a succession of phases, both in physiological function and capacity, and in morbid imminence. It is incorrect to assert that at birth the child is an entirely imperfect being. The functions of capillary circulation, of molecular nutrition, of urinary excretion, are performed with more surety and vigor than at many other periods of life. The dangers arising from novelty of function in the respiratory apparatus and the great susceptibility to cold are largely compensated by the peculiarities in the relations between the heart and the lungs. During infancy the pulmonary artery remains wider than the aorta, and pressure in the pulmonary exceeds that in the systemic circulation. Extensive obstruction to the circulation, with obstacle to the work of the right heart, occurs, therefore, less frequently than in adult life, where the conditions are reversed, and the force of the pulmonic circulation falls below that of the systemic. Hence paralysis of the heart from mechanical causes, as the obstruction to the circulation caused by pneumonia, is less liable to occur: and thus pneumonia is really a less dangerous disease in children than in adults.

During fœtal life marked inequality exists in the distribution of oxygenated blood. This comes from the placenta by the umbilical vein, and, brought to the heart by the inferior vena cava, is carried directly across the right auricle to the foramen ovale by means of the Eustachian valve, that really forms a special channel for its conduction. It is this blood which reaches, almost unmixed, the left ventricle, to be thence thrown through the aorta and the vessels springing from its arch towards the cephalic extremity of the fœtus, towards the brain, and more especially the medulla, with its important cardiac and respiratory centres. These, therefore, get a special start in development. On the other hand, the venous blood returned from the head and the upper extremities enters the right auricle in front of the Eustachian valve, or channel, and, by the pulmonary artery and ductus arteriosus, reaches the descending aorta, to be distributed unmixed to the lower half of the body. This, therefore, remains retarded in development as compared with the upper half, and not only the limbs, but the lower portion of the spinal cord, with the ganglionic centres governing the limbs and the pelvic organs.

Now it is a fact that the lumbar portion of the cord and spinal column show, certainly throughout childhood, a morbid imminence superior to that of the upper portions. Osteo-myelitis, in the one, anterior poliomyelitis in the other are much more frequent in the lumbar than in the dorsal or cervical regions. We have already alluded to the predilection of spina bifida for this region, where the vertebral arches are the last to close. May we not consider all these facts as the consequences of a nutrition, defective in comparison with that of the upper nerve centres and spinal column? We may even go further and trace to such disproportion, failing to lessen, or even increasing, with advancing years, such imperfect development of the utero-ovarian system in many anæmic children as entails disease as soon as these organs enter upon function. If the maximum nutritive currents of the body only just touch par the minimum cannot fail to fall below the level of healthy vitality.

The foregoing considerations are theoretical. Practical precautions of great importance are required in the adjustment of external media to the unequally unfolding organism of the child. The adjustment of foods to the successive phases of development of the digestive organs; of effort to those of the locomotor appara-

tus; perhaps, most difficult of all, of action and rest to the complex phases in the development of different parts of the nervous system. It would carry us much too far to show how profoundly this principle enters into the guidance of mental development; and how much it has been overlooked until most recent times. The child's mind and body for centuries has been looked at simply as a miniature of the man's; the fact that the inter-relation of their parts was differently proportioned has, until now, been most dimly apprehended.

The final principle to which I would call your attention is that of the mutual limitation of parts. In the embryo the natural termination of the growth of any mass of cells is effected when it is brought in contact with an opposing mass, growing with equal or superior vigor. Failure in the establishment of such limiting contact results in the undue growth of the first mass of cells. An interesting illustration of such failure drawn from infantile pathology is suggested by a remark of Bouchard. This author would explain the excessive growth of the medulla, of bones in rachitis, by the defective calcification of the bones. The medullary elements, insufficiently imprisoned in calcareous envelope, are left free to develop immoderately.

Diminution in the calcareous matter of long bones, occasioned by prolonged febrile diseases, may similarly help to explain the growth of the patient, which is often observed in convalescence from such diseases. The medullary elements undergo a nutritive irritation in virtue of the febrile disease; this coinciding with a diminished resistance in the osseous envelope is liable to determine in adolescents a sudden increase of growth, which is not of good augury, inasmuch as nutritive material is thus withdrawn from nervo-muscular tissues greatly in need of repair. This same reason justifies the popular dread of "outgrowing the strength," a process frequently observed in delicate children or youth. When, moreover, with elongation of the skeleton the heart and lungs fail to grow in proportion; when, by increasing length of the neck, the brain is removed farther from the heart, and thus receives its impact of blood less forcibly, muscular weariness, anæmic headaches soon result.

These few examples may serve to illustrate a principle whose application is probably far-reaching. It is possible that the primary reason for the development of neoplasms consists in a

failure of normal tissues to limit the growth of certain elements either homogeneous or heterogenously imprisoned in the organism at birth. (Cohnheim.)

The foregoing most cursory survey briefly indicates the line of thought which seems to me most profitable to pursue in studying the diseases of children. With all considerations drawn from general pathology are to be combined those based upon the special fact of continuous growth of unequally rapid evolution. The morbid conditions altogether peculiar to childhood are to be understood only by reference to circumstances of development. Study of disease common to children and to adults must be outlined in its main features in the latter, but then specially adapted to the former, and peculiarities explained by, again, peculiarities in the phase of development. And to thoroughly understand the meaning of developmental processes these must be studied where they are most rapid and their results most vivid and striking, namely, in the embryo and fœtus. Here may be first acquired the full conception of the wonderful flexibility of living things, of the ceaseless rush of life towards its goal; of the curve of changes so incredibly rapid at the outset, so gradually slackening throughout childhood towards the relative stability of adult existence, to finally recommence, substituting for the original increment a constant decrement of force, until the wave of life terminates in death.

THE INDICATION FOR QUININE IN PNEUMONIA.[1]

MR. MILL closes a review of the poems of Alfred de Musset with the remark, "How much life is required to produce a little poetry!" Similarly, I think, no one can sift clinical records without feeling inclined to exclaim, "What an enormous amount of data are required to justify a few positive conclusions!" On this account, fresh observations are always in order, and this is my excuse for presenting the partial conclusions which may be deduced from a hundred dispensary cases of pneumonia treated by quinine.

It would seem, at first, as if 100 cases would suffice for many and quite positive conclusions. But the uncertainty of dispensary practice is so great that, out of this 100, only 33 cases were followed to complete termination. In 20 others the record continues until after marked defervescence had occurred, making a fairly satisfactory total of 53. In 20 other cases the record stops during the continuation of fever or of marked physical signs, while in 28 cases the patients were only brought to the dispensary on a single occasion. The age of the patients varied from three weeks to eight years, much the larger number being about two years and a half old.

From the incompleteness of so many of the histories, it is useless to attempt positive conclusions in regard to the mortality statistics. Out of the whole number, seven are known to have died, so that the mortality can not be less than seven per cent., while it may be greater. But, on the other hand, the conditions were so generally unfavorable that it is impossible from these data to estimate the chances of death from the disease under quinine treatment, when all the details of management should be under the physician's control. Of course, all the cases brought

[1] Reprinted from *The New York Medical Journal*, 1887.

to the dispensary were, by that fact, submitted to a degree of exposure which would be carefully avoided in either hospital or private practice. The general hygiene of the house was usually bad, the nursing unskillful and often inattentive, and a tendency to caseation and tuberculization existed very frequently, either constitutional or as a result of measles. One of the seven deaths occurred in a pneumonia following diphtheria; in another case it was consecutive to scarlet fever. Setting aside the mortality statistics as entirely inadequate for any useful purpose, the data may be utilized in the inquiry how far the fever or the physical signs of pneumonia are demonstrably influenced by quinine.

The physical signs especially investigated were the combination of extremely harsh or of bronchial breathing with dullness on percussion. In a few cases tympanitic percussion sound existed over areas where the auscultatory sounds would have led the observer to expect dullness. This paradoxical phenomenon is best explained by the German theory, which ascribes it to some such infiltration of the walls of the air-cells as may prevent them from vibrating under percussion. The column of air then vibrates alone—gives rise to homogeneous vibrations, as shown by experiments with the sensitive gas-flame—and such homogeneous vibrations have a tympanitic resonance. Similarly, percussion over the stomach normally occasions a tympanitic sound, because the walls are too flaccid to vibrate. But if the stomach be first overdistended, or, conversely, if healthy lungs be removed from the body, and percussed in their relatively collapsed condition, the note becomes duller in the first case, tympanitic in the second. Upon inflating the lungs, however, and repeating the percussion, the tympanitic note is found to have disappeared.

This experiment is mentioned by both Weil and Gerhardt in their treatises on percussion and auscultation. I have repeated the experiment, and obtained exactly the results stated. I have made somewhat of a digression to mention these facts, because I have found them omitted by many English and American authors.

Clinically, tympanitic percussion sound with consolidation often coincides with caseous degeneration of the lung, and the theory would therefore be sustained by the peculiar infiltration

of the alveolar walls known to characterize Buhl's desquamative pneumonia.

To return to the clinical analysis. Seventeen of the cases examined had blowing respiration in some portion of the lung, usually in the upper scapular region, and among these 16 had fever. Forty-eight cases exhibited tubular breathing, and among these only 2 were without fever at the time treatment was begun.

To nearly all these cases quinine was administered in about the same way. Five grains were given at night, and the same amount in the morning, each in two doses at an hour's interval. In babies under eight months old, two grains and a half were given night and morning, similarly divided. In one or two cases requiring special mention, to children over two years old, larger doses were given. Out of the 16 cases of the first class, with harsh and blowing respiration, usually, of course, accompanied by râles, and other signs, physical and rational, that I need not here particularly describe, the blowing respiration disappeared—

After	2	days	in	3	cases;
"	3	"	"	2	"
"	4	"	"	6	"
"	5	"	"	1	case
"	6	"	"	1	"
"	7	"	"	1	"
"	14	"	"	1	"

In the apyretic case with blowing respiration this had disappeared after two days' medication.

In the second class, where tubular breathing indicated more intense congestion, or even consolidation of the lung, this was relieved, as shown by the disappearance of the sign—

After	2	days	in	3	cases;
"	3	"	"	3	"
"	3	"	"	3	"
"	6	"	"	1	case;
"	7	"	"	5	cases;
"	11	days	in	1	case;
"	14	"	"	1	"
"	17	"	"	1	"
"	18	"	"	3	cases;
"	3	weeks	"	1	case.

Two cases, without fever, lost their tubular breathing in one and ten weeks respectively.

Twenty-four cases were not observed to complete termination, the patients ceasing attendance. In 10 of these the fever had almost disappeared, and the patients were greatly improved when lost sight of; there was every reason to believe that they completely recovered. But in these, when last seen, the tubular breathing was persisting—

After	2	days	in	3	cases;
"	6	"	"	1	case;
"	7	"	"	2	cases;
"	9	"	"	1	case;
"	12	"	"	1	"
"	18	"	"	1	"
"	6	weeks	"	1	"

Among the patients lost sight of while the fever was still high, and the morbid process progressing, the tubular breathing was persisting—

After	2	days	in	6	cases;
"	3	"	"	1	case;
"	4	"	"	1	"
"	5	"	"	1	"
"	7	"	"	2	cases;
"	10	"	"	1	case;
"	11	"	"	1	"
"	12	"	"	1	"

The tables of cases in which the patient was under observation until the physical signs had been effectively modified show that this modification occurred at varying intervals during the first week from beginning of treatment in 29 cases, and in from eleven to eighteen days in only 7 cases; finally after three weeks in 1 case. On this account, the persistence of tubular breathing during from two to seven days in 17 cases, which were not watched to their termination, proves nothing against the final recovery of the patients. Such recovery was almost certain, moreover, in 6 of these 17 cases, because, notwithstanding the persistence of tubular breathing, marked defervescence had occurred, and the patient was evidently entering upon convalescence. The possible occurrence of fresh attacks, or of extension of the morbid process to other parts of the lungs, would prove nothing against the favorable modification of the first set of symptoms by the treatment.

In 10 cases tubular breathing developed while the patient was receiving quinine. The treatment had been instituted when the occurrence of hurried and harsh respiration, together with fever, had pointed to a commencing broncho-pneumonia in scattered or in deep-lying foci. The appearance of the tubular breathing after two or three days of quinine medication indicates that in these cases the extension of the morbid process, or the agglomeration of the pneumonic foci, was not averted by the treatment. In 4 of the cases the lungs cleared up on the fourth, seventh, tenth, and fourteenth day, respectively. In 6 the patients were lost sight of on the second, fourth, fifth, and seventh day, and the tubular breathing persisted. To sum up such results as were obtainable from the data, we may say that quinine was given in 59 cases which could be watched to the beginning of convalescence or to death. Seven cases proved fatal, either during the height of the acute attack or by the supervention of acute tuberculization.

In 29 cases blowing or tubular breathing disappeared in from two to seven days, in 7 cases in from eleven to eighteen days, in 1 case after three weeks, thus making a total of 37 cases.

Finally, in 10 cases, though defervescence was established, the tubercular breathing was persisting in from two days to six weeks; thus in about one fourth as many cases as those in which it disappeared. But these 10 cases represented convalescence, and may be added to the 37 cases in which the physical signs disappeared with the fever. In addition to the pyretic cases, quinine was given in 5 apyretic cases of chronic consolidation of the lung, and in one of congestion from mitral insufficiency.

As these cases bear particularly upon the view of quinine to be advocated in this paper, a summary of each will be given.

Case XXXVIII.—Two and a half years; broncho-pneumonia fourteen days after measles; first seen May 10th; harsh and laborious respiration at right apex; t. 100.5°. ℞ Vin. seneg. and am. carb.

May 12th.—No improvement. ℞ Potass. chlor., sod. bicarb., syr. ipecac., inf. prun. virg.

15th.—Abundant râles upper half right lung; respiration very harsh; expiration blowing at apex. ℞ Poultice. Quinine, gr. v, night and morning, in two doses at an hour's interval.

19th.—Vomited after quinine; râles diminished; expiration still blowing; t. 100°. ℞ Quin., gr. ij, every two hours.

22d.—Much improvement in general appearance; no fever, no râles, no vomiting; expiration, however, tubular, at apex. Continue quinine, but in five grains, night and morning, as on 15th.

The treatment was continued with progressive improvement until on June 9th the tubular breathing was found to have disappeared. The quinine was then suspended. ℞ Syr. tolu.

June 16th.—Return of cough; tubular breathing found at left apex. Resumed quinine; continued until—

30th.—When tubular breathing entirely gone. Quinine suspended.

Case LIV.—Child two years; first seen December 4th; pneumonia lasting since measles in September; dullness and increased vocal resonance at both apices; whiffs of tubular breathing; t. 99.5°; glands of groin enlarged. ℞ Ol. morrhuæ and calcii phosphatis.

January 29th.—Child reported as having entirely recovered. Then acute attack, ushered in by convulsion; t. 102.5; R. 48; harsh respiration over both lungs without râles. ℞ Quin., gr. v, morning and night, in two doses each time.

31st.—T. 99°; R. 36. Continue quinine.

February 5th.—Respiration harsh and blowing at both apices. Continue quinine, also ol. morrhuæ and calcii phos.

19th.—Respiration nearly normal. ℞ Quin., gr. ijss., night and morning. Tinct. capsici externally.

March 12th.—Recovery complete.

Case VII.—Chronic pneumonia or peribronchitis; boy aged ten. On August 31st tubular breathing at right apex; no fever. ℞ Quinine, gr. v, night and morning.

September 23d.—Only occasional whiffs of tubular breathing in right supra-spinous space. Continue quinine.

October 12th.—Respiration only tubular on forced breathing.

Case XXXVI.—Aged two and a half years; cough for four months; slight tubular breathing at apex of right lung; t. 100°; on October 25th, quin., gr. v, night and morning in divided doses.

27th.—No tubular breathing; respiration harsh; t. 98.5°; apparent improvement.

CASE LVI.—Child aged three months; cough for five weeks; bronchial breathing with dullness at right apex. On February 5th, quinine, gr. ij., night and morning.

February 12th.—Tubular breathing at right apex. Continue quinine, also ol. morrhuæ and calcis chlor.

19th.—Cough lessened. Continue.

23d.—Tubular breathing gone; replaced by harsh respirations, with subcrepitant râles.

CASE VI.—Boy aged nine months; rheumatic mitral insufficiency without hypertrophy; much dyspnœa and cough. Presumed hyperæmia of bronchial mucosa. Digitalis for three days, without obvious effect. Then quinine, gr. v, night and morning. Three days later child much improved; no cough; cheeks and lips red instead of bluish.

In 32 cases the fever was observed to entirely disappear, or to fall to 101°, under the quinine medication. This defervescence occurred—

On	the	2d	day	in	8	cases;
"	"	3d	"	"	3	"
"	"	4th	"	"	3	"
"	"	5th	"	"	5	"
"	"	6th	"	"	2	"
"	"	7th	"	"	4	"
"	"	8th	"	"	2	"
"	"	10th	"	"	1	case;
"	"	11th	"	"	1	"
"	"	12th	"	"	1	"
"	"	13th	"	"	1	"
"	"	19th	"	"	1	"
					32	cases.

Thus in 25 cases defervescence occurred within seven days after beginning the quinine; in seven cases it was later. In 14 of the 32 cases the physical signs persisted though the temperature fell, 12 of these cases having tubular breathing.

In the remaining 18 cases the physical signs disappeared, or were greatly modified, coincidently with the fall of the temperature.

In 19 cases more or less fever, sometimes as much as 105°, was found on the last visit of the child, which occurred—

After	2	days in	7	cases;
"	3	" "	1	case;
"	4	" "	2	cases;
"	5	" "	2	"
"	7	" "	1	case;
"	9	" "	1	"
"	10	" "	2	cases;
"	12	" "	3	cases.

In 4 of these 20 cases the physical signs had greatly improved, although the fever persisted. In the remaining 16 the physical signs also remained unchanged. Comparison of this table of 19 apparently unfavorable cases with the table of 32 cases in which defervescence was positively observed somewhat diminishes the unfavorable aspect of the incomplete cases. For a delay of defervescence as far as the seventh day, observed in 13 of the latter class, is not at all exceptional in cases of perfect ultimate recovery, for it occurred in 25 out of the 32 undoubtedly successful cases. The remaining 7 of these, moreover, delayed defervescence as long as, or longer than, the remaining 6 of the second division.

The temperatures existing at the time the quinine treatment was begun varied from 103° to 105°. Within this range the height of the temperature did not seem to modify its resistance to medication. On the other hand, an increased amount of quinine did not seem to exercise any greater influence over the fever than the ten grains a day habitually given.

Thus, in one case early in the series a child of six months received, in divided doses, ten grains of quinine in the evening and five in the morning. This was well tolerated, and, on reporting two days later, the child seemed much better, the respiration was easy, and the temperature 100°. Tubular breathing was heard bilaterally at the root of the lungs, and the percussion resonance was diminished over both lungs. The quinine was reduced to five grains night and morning. Two days later the temperature rose to 105°, the tubular breathing had disappeared, but the child was vomiting and the amount of urine diminished. Quinine was given by rectal injection, and again to the amount of fifteen grains in twenty-four hours. It was nearly all rejected, but on the two following days ten grains were administered in two doses by the mouth after two days; the temperature was still at 103°, the respiration 60, and the pulse 140.

The mother then ceased attendance and medication for a week, rather wisely as it proved, for at the end of that time the baby was in full convalescence, the temperature 101°, the respiratory signs almost normal.

The foregoing case is, I think, of value, purely as a physiological experiment, in showing the resistance of pneumonic fever to doses of quinine which might be considered overwhelming for a child of six months of age. Another case, not included in the foregoing list, exhibits the same resistance of febrile temperature to larger doses of quinine under somewhat different circumstances.

A boy of four years entered the infirmary with a history of pneumonia following measles, and of four weeks' duration. Over the upper fourth of the right lung and nearly the whole of the left the respiration was extremely harsh in both times, with the expiration prolonged but not tubular. Moderate flatness on percussion existed over the upper fourth of both lungs, but over the middle third of the left was a markedly tympanitic resonance, prolonged into the axillary space. Notwithstanding this situation, which seemed favorable to the theory of tympanitic resonance from the stomach, I regarded the sign as probably indicative of commencing caseous infiltration. The child at first received five grains of quinine night and morning, and this was continued several days as an experiment, and to ascertain if the signs of pulmonary congestion would be modified by larger doses of quinine. Twenty grains of the drug were administered in twenty-four hours, in divided doses every six hours. During the second twenty-four hours of this medication, which was apparently well tolerated, the temperature rose to 102.5°, and continued to rise every evening after the larger doses of quinine were stopped.

The hectic thus developed was attributable to the tuberculization of the lung that became soon manifest; but it was clear that the fever was not even symptomatically checked by the quinine.

In a third case, a baby four months old, with signs of bronchopneumonia at the apex of both lungs and a temperature of 102.5°, received a grain of quinine every two hours. Much of this was vomited. Three days later the temperature was still at 102°, and the physical signs unchanged. The use of quinine was interrupted for two days, then resumed as before, together with

camphor and brandy. The temperature was then 103°. Two days later this temperature was unchanged, though the quinine had been retained. The tubular breathing, with râles, had extended to the middle of the right lung. Three days later the temperature was 106°. The left apex and the upper half of the right lung solidified.

At this point the child was lost sight of. Whether, as was only too probable, it died could not be ascertained; but it is certain that the large doses of quinine entirely failed to prevent either the rise of temperature or the extension of the morbid process in the lungs. Between the extreme and special cases just mentioned, and which in this series are the only ones recorded as those in which more than ten grains of quinine were given daily, and the mass of the cases in which precisely this amount was given, lies a considerable range of possibilities for varying methods of medication. Into these, of course, the reasoning of this paper can not enter. But, from the data given, the positive conclusion can be drawn that, in the pneumonia of children between six months and four years of age, a daily dose of ten grains of quinine does not act as an antipyretic *per se*, does not reduce temperature directly and apart from the morbid process, but only in so far as it modifies this.

It is true that in fourteen out of thirty-two cases defervescence occurred while signs of consolidation still persisted in the lungs. But it is well established that such signs do not contradict the arrest of the morbid process, but merely indicate the continued presence of the exudation which this has already caused. In croupous pneumonia the exudation signs may persist long into well-established convalescence. In catarrhal pneumonia, and especially in children, the exudation can not persist without risk of caseation; but this is a new process, and its dangers need not be immediately taken into account in the management of acute inflammation. Defervescence, therefore, except when brought on purely symptomatically—as by cold, or large doses of antipyretics— always indicates at least a temporary arrest of the morbid process, while the disappearance of many signs of congestion, which in children so often simulate those of exudation, indicates no convalescence so long as the temperature remains elevated. A forced defervescence in broncho-pneumonia, apart from an arrest of the morbid process, would be of little advantage

to the patient, unless in the relatively few cases where a great excess of temperature threatens or causes convulsions. There is certainly no danger in children, in non-infectious pneumonia, of cardiac degeneration from either heat or other cause. The pneumonias after diphtheria or scarlatina may often owe their fatal termination to infectious myocarditis, with thrombosis; but against this danger antipyretics would be powerless. Apart from the possibility of an infecting agent in the blood, the fever in pneumonia seems most plausibly explained by paresis of the heat-regulating centres of the medulla, the consequence of an excess of excitations conveyed to the medulla from the lungs in the centripetal fibres of the vagus. According to the experiments of Rosenbach [1] in normal respiration, the distension of the lung in complete inspiration irritates the pulmonary fibres of the vagus; the irritation is conveyed along these fibres to the medulla, and inhibits the excitation of the inspiratory centre, which, determined by the presence of carbonic acid in the blood, has been the cause of the inspiratory expansion of the thorax. The irritations thus generated in the lung convert the otherwise permanent excitation of the inspiratory centre into a rhythmical excitation by means of periodical interruptions. When the inspiratory centre is inhibited, the inspiratory movement is arrested, the thorax passes into the passive state of expiration, and remains there until the increasing venosity of the blood again excites the inspiratory centre.

When the pulmonary tissue is inflamed, the fibres of the vagus suffer an abnormal irritation; the medullary center is therefore more frequently inhibited; the respiratory movements are shorter and far more frequent than in health. A sudden acceleration of respiration, with a rise of temperature, may often suffice to indicate inflammation of the lung tissue, or the transition from bronchitis to pneumonia, even in the entire absence of physical signs.

The centripetal nervous irritations which suffice to inhibit the respiratory centre and the heat-regulating centres of the medulla can hardly fail to affect its vaso-motor centre as well. It seems, indeed, highly probable that the vasomotor fibres of the pulmonary blood-vessels, whose independent existence has not been demonstrated, run in the paths of the

[1] *Studien über den Nervus Vagus*, Berlin, 1877.

vagus. But the immediate effect of irritating the vaso-motor centre is the contraction of blood-vessels. The dilatation of pulmonary blood-vessels in pneumonia can only be referred to the vaso-motor system under two circumstances. First, when the pulmonary congestion is generalized and primary, dependent on some overpowering influence primarily exercised on the vaso-motor centre and apart from local pulmonary disease. Such congestions are seen in malarial and other infections. In the second case, the long-continued irritation of the vaso-motor centre, when the periphery of the vagus has been severely irritated by inflammation of lung tissue, may result in secondary paresis of the medullary centre, and hence in the terminal congestions of fatal cases of pneumonia.

It is worth noting that two out of the three immediate consequences of vagus irritation in pneumonia are conservative in their tendency. The increased frequency of respiration tends to compensate the restriction in area of respiratory tissue, and this has frequently been pointed out. Irritation of the vaso-motor centre tends to antagonize the tendency to abnormal dilatation of blood-vessels caused by the unknown irritament of the inflammation.

It has not yet been demonstrated that the fever which results from inhibition of the heat-regulating centres is also conservative, and only dangerous when in excess, but the other facts render this *a priori* probable. Diminution in the area of respiratory tissue threatens ultimately a diminution of the oxidations upon which the vital heat depends. The rise of bodily temperature caused by deficient elimination of heat—the probable cause of the fever in at least catarrhal pneumonia—seems to indicate a blind effort on the part of the organism to husband its heat resources, and thus to avert the collapse which is threatened by the disease.

The theoretical estimate of the action of quinine, or of any medication, in pneumonia must be based on the manner in which it can be shown to affect these vagus irritations, or else the pulmonary lesions which occasion them.

The most usual accounts of the lesions of broncho-pneumonia given by English and American writers emphasize the existence of bronchitis, and then describe the peri-bronchial alveoli as in a state of catarrhal inflammation, characterized

by a proliferation and desquamation of epithelial cells. Delafield,[1] however, describes hepatized lobules—not granular, as in croupous pneumonia—yet standing out above the surrounding tissue, and filled with epithelium, leucocytes, and fibrin. This fibrinous exudation, according to Charcot and Grancher—the French writers who have most recently investigated the subject—is particularly characteristic of the peribronchial alveoli at the centre of the lobule. Thus, as Cadet de Gassicourt remarks, we may no longer establish a distinction between croupous and catarrhal pneumonia, according to the presence or absence of fibrin in the exudation.[2] This central zone of hepatization, the peribronchial nodule, is surrounded by a zone of splenization. This is caused by a congestion of the alveolar walls, or even by a beginning of infiltration of these walls by embryonic cells, and by a mass of voluminous epithelial cells with a few leucocytes in the interior of the cells. This zone may be entirely absent, or it may greatly predominate. "It plays an important rôle in the clinic, for it belongs at once to hyperæmia and hepatization; it is mobile like the one, and fixed like the other, and may sometimes extend with great rapidity." [3]

The localization of the most severely injured tissue in bronchopneumonia, in the immediate vicinity of the inflamed bronchus, would seem to confirm the old doctrine that the irritament, instead of being diffused, as in croupous pneumonia, is brought to the air-cells by the bronchi, or generated among the products of the inflammation of their mucosa. The condition of the outer zone of the lobule indicates a more diluted action of the same irritament, one of whose effects is the formation of leucocytes—*i. e.*, of pus; the other, the paralysis of the capillaries. The way is clearly open for the future demonstration of some form of bacteria which shall have been cultivated in the mucus of the inflamed bronchial tubes, and secreted the poison that could produce these characteristic effects. Such a bacterium, the pneumococcus of Friedländer, is known to have been accepted by many authorities as the efficient cause of croupous pneumonia, though Sternberg considers it identical with a micrococcus existing in the saliva. I am not aware that any

[1] *Studies in Path. Anat.* vol. i, p. 67.

[2] *Maladies de l'enfance*, vol. i, p. 153, Paris, 1880.

[3] Cadet de Gassicourt, *l. c.*, p. 152.

discovery analogous to Friedländer's has been made for broncho-pneumonia; we must therefore continue to speak of an unknown irritament as the cause of the vascular and other lesions of the disease.

In our ignorance of the precise nature of the irritament, or of the manner in which it determines the pulmonary lesions of the disease, our therapeutic action must be indirect. It must aim at maintaining or at restoring the circulation on the periphery of the inflamed lobules, or of an aggregation of them. This aim is partly accomplished by the systematic application of moist heat, and the use of such remedies as seem to modify the bronchitis, and there is much reason to believe that, for direct action upon the congested pulmonary blood-vessels, quinine is the best drug at present known.

I do not propose in this place to review the enormous literature, experimental, critical, and clinical, which exists on the subject of quinine. I wish rather to call attention to one theory of its action which seems to me to have been too much neglected. I refer to the theory advocated by the Neapolitan writer Chirone, in an experimental essay published in the *Gazette hebdomadaire* for 1875.

Chirone proposed to find some method for reconciling two current doctrines concerning quinine, each imposingly supported, yet apparently incompatible with each other. According to one of these doctrines, quinine is a powerful sedative to the heart and nervous system. According to the other opinion, and an extensive clinical experience, quinine is a powerful tonic to both nervous system and heart. It is agreed that these different effects are obtained by means of very different doses, being large in the first case, moderate in the second. Still, it is important to ascertain the precise point at which the tonic effect passes over into the sedative and depressing effect, and the mechanism by which this may be determined. The phenomenon only remotely resembles the contrast which is afforded by minute and average doses of other alkaloids, as morphine.

Experiments on dogs, rabbits, and frogs led Chirone to the following conclusion:

Quinine increases the diastole of the heart through a direct molecular action on the muscular fiber, in virtue of which this actively lengthens and the cavities it incloses become thereby

enlarged. From the increased energy of diastole more blood is aspired to the heart, and, in order to empty itself, the organ is compelled to contract more energetically, and thus the systole is indirectly increased in power. This is the case so long as the dose of quinine is moderate; the total result, therefore, is an increased energy of the circulation with consequent tonic effect.

If the dose of quinine is large, however, the cardiac diastole comes to predominate too much over the systole; more blood is aspired into the heart than can be expelled; the ventricular systole struggles in vain with the load, finally becomes paralyzed by it, and the heart stands arrested in diastole, with its cavities enormously dilated by blood. In the experiments on dogs, where the carotid tension was measured by Fick's manometer, the constant tension began to fall after the injection into the jugular of three doses of quinine of 15 centigrammes each; it soon completely disappeared. In the experiments on the frog the heart was exposed and observed for some time, the diameter of the base measured at the moment of maximum diastole. This, in one experiment, for instance, was 7.2 millimetres. Eight centigrammes of bisulphate were injected under the skin. In three minutes the heart was observed to dilate very energetically, to become very red. In ten minutes the ventricles were arrested in diastole, and their base measured 9.3 millimetres. The auricles were still beating. That this diastole was active and not cadaveric, the author infers from the fact that in thirty minutes the base measured only 8.6 millimetres and retained this size an hour later when quite dead. During this diastolic arrest the heart failed to respond to electricity. But when, in another experiment at the moment of increased diastole, a few drops of a solution of toad venom were injected, the systole grew gradually stronger, gained upon the diastole, until after energetic systolic contractions, in thirty-five minutes, the heart stood arrested in systole.

The toad venom has the property of stimulating the systolic contractions of the heart, and its ability to act upon a heart which is under the influence of quinine proves that this latter drug has not abolished contractility, but only stimulated the movement of the cardiac fiber in the direction of its extensility. The antagonism of the two poisons by means of action upon dif-

ferent mechanisms is analogous to the cardiac antagonism which exists between muscarine and atropine. I have repeated the experiment on the frog, with similar results.

A frog at 10.30 received 8 centigrammes of bisulphate and a second similar dose at 10.41. Just before the injection the heart was contracting at the rate of 30 beats a minute; the base in maximum diastole measured 9 millimetres. Within three minutes after the injection the cardiac cycles became intensified, the diastole increased, the systole also energetic, but no longer rendering the ventricle white. In nine minutes the base diameter had decreased to 8 millimetres. In fifteen minutes the base had increased in diastole to 11 millimetres; the number of beats had fallen to 15 in a minute.

In a second frog, previous to the use of quinine the heart beat at 30 in the minute, becoming completely white in systole; the maximum diameter of the base measured 7 millimetres. Eight centigrammes of bisulphate were injected in two doses. In four minutes the beats had fallen to 24, in eight minutes to 22, and the base measured 9 millimetres. At this time the ventricle exhibited isolated tonic contractions by which it was divided in three parts. A similar observation has been made by Chirone.

In twelve minutes the heart remained red throughout systole; the base measured 10 millimetres. In nineteen minutes the base measured 11 millimetres; the number of beats had fallen to 16.

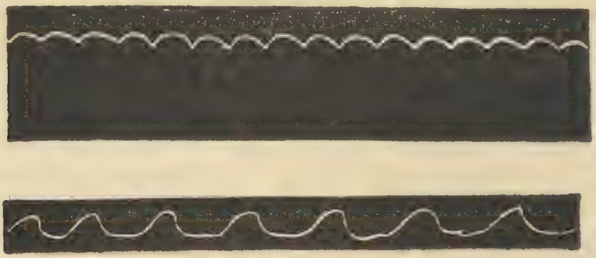
It is noteworthy that when monosulphate of quinine dissolved in water by means of acid was used, the heart was arrested in diastole in less than five minutes. The effect is here attributable to the acid rather than to the quinine. Only the soluble bisulphate is suitable for the experiment.

In addition to the experiments intended to directly measure the size of the heart in diastole and which I have imitated from Chirone, I have been able to make others which exhibit the influence of quinine on the cardiac tracings, obtained by means of a lever. For this purpose a frog, previously quieted by a quarter of a milligramme of woorara, was attached to the frog-plate and the heart exposed. A light lever, composed of a straw and an exploring disc of pith, reposed on the heart, and registered its movements on the revolving cylinder. The first trace is

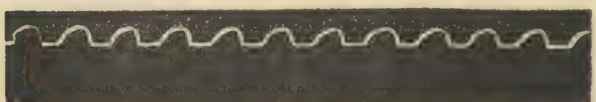
the normal trace of the same frog to which quinine was subsequently administered. The pericardium was intact, and on this account the excursions of the lever are less ample than in the other tracings. The heart beat thirty times in a minute. To the same frog was then administered 0.08 centigramme of bisulphate of quinine, and the first tracing taken ten minutes later. An immediate and noticeable change took place in the tracing. The stroke marking the systole of the ventricle is 3 millimetres high and 5 broad, while before the quinine it was 2 millimetres high and 5 broad. The systole therefore is more energetic, but no more prolonged. The diastole, on the contrary, is markedly prolonged, being scarcely measurable on the normal trace, while a perfectly straight line of 3 millimetres in length measures it on the quinine tracing. The extreme shortness of the diastole on the pre-quinine trace is not always observed, but quite the contrary, as may be shown by the other normal tracings taken from another frog. But the peculiar abruptness of the diastolic line and general appearance of the tracing following quinine was not observed except under its influence. At the time of this first tracing, ten minutes after the use of quinine, the heart did not offer the characteristic appearance that had been previously noticed—that is, the ventricle did not remain red during systole, but contracted completely, and became entirely white as usual. The number of beats was still thirty in a minute.

The tracing rate of pulsation and other characters remained the same for twenty-four minutes from the time of injection. Then (as shown in the succeeding tracings) when the ventricle began to remain red during systole, the diastole was markedly increased, and the character of the tracing changed. The curve of the systole is the same height—3 millimetres—but the ascent and descent are both gradual instead of so peculiarly abrupt, and the space occupied is 6 millimetres. The diastolic straight line has become a rounded curve, whose extremities are 5 millimetres apart. The diastole is still shorter in time than the systole, but it is increasing in energy, as shown by the sinking of the exploring disc into a curve, instead of a straight line. The rate of pulsation is only 18 to a minute. A strip of tracing 8 centimetres long contains only 9 pulse tracings instead of 17. Nine minutes later and the height of the ventricular systole trace

reached a maximum of 4 millimetres, while retaining a breadth of 6 millimetres. The duration, therefore, was the same, the energy still increased; the diastole is a little shortened. In forty-one minutes after the injection the systolic tracing begins to fall in height; in fifty-one minutes the systole is markedly enfeebled and the diastole apparently prolonged, though not really more so than on the previous tracings.



TRACE I.—(*a*) Tracing of normal frog's heart, taken *in situ;* beats, 30; $\frac{1}{4}$ millig. woorara. (*b*) Tracing from another frog's heart.



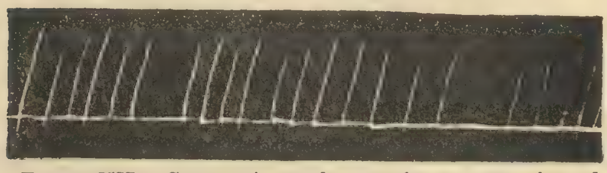
TRACE II.—Cardiac tracing from heart (*a*) 10 minutes after hypodermic injection 0.08 bisulphate quinine; beats, 30; time, 5.12.

TRACE III.—Same at 5.22.

TRACE IV.—Same at 5.26; 18 beats to minute; heart red during systole.

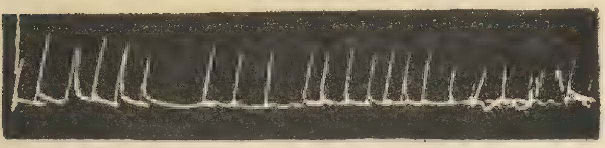
TRACE V.—Same at 5.35.

TRACE VI.—Same at 5.56. Observation ceased.

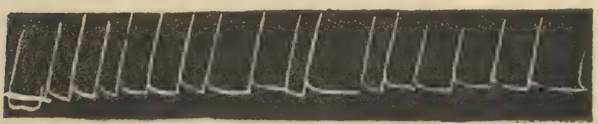


TRACE VII.—Contractions of normal gastrocnemius of frog, in Pfluger's myrograph, with stationary cylinder; weight, 20 grammes; R. A., 220 millimetres; time, 3.40; height of first 40 contractions, from 15 to 6 millimetres. Circuit closed by hand.

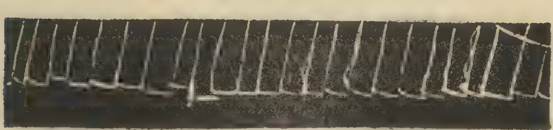
TRACE VIII.—Same at 3.47; 96 contractions obtained in 7 minutes before exhaustion. Rest of 1 minute included in this time.



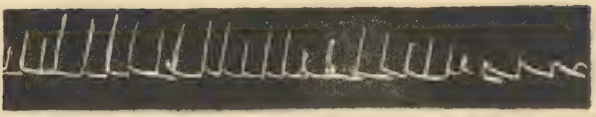
TRACE IX.—First contractions gastrocnemius 37 minutes after hypodermic injection of 0.08 bisulphate quinine; cylinder stationary; weight, 20 grammes; R. A., 210 millimetres; exhaustion in 2 minutes, after 61 contractions; height of first 40 contractions, from 10 to 3 millimetres. Circuit closed by hand.



TRACE X.



TRACE XI.



TRACE XII.—Same muscle after rest of 9 minutes; exhaustion in 2 minutes, after 54 contractions; height of first 40 contractions, from 10 to 5 millimetres.

In fifty-four minutes the diastolic tracing becomes again a straight line, but now, for the first time, 6 millimetres in length. The duration of the systole was now expressed by a space of 5 millimetres, so that the diastole had decidedly gained upon it.

The measurements are the same on the last tracing taken fifty-eight minutes after the use of quinine. The observation was then interrupted.

The tracings entirely confirm the inferences drawn from the direct observation of the heart contracting under the influence of quinine. They show the increased length and energy of the diastole, but they show also that this is not a proof of paralysis; for a considerable time the energy of the systole is correlatively increased. The diastole finally gains on the systole, and the heart pauses in diastolic arrest. That the diastole of the heart is as distinctly active as the systole, has been sustained with weighty argument by Pettigrew.

"There is every reason," says this physiologist, "to believe that the movements of the amœba and the sarcous elements of a muscle are identical. Both can change their form; elongation in one direction entailing shortening in another and opposite direction. . . . The movements of the amœba are doubtless referable to a centripetal and centrifugal power inhering in the protoplasmic mass which enables the creature to advance or elongate, and withdraw or shorten, any part of its body. At times the amœba elongates its entire body by a wavelike movement, after which it sends out lateral processes which exactly correspond with the bulgings produced on a muscular fibre when it is made to contract or shorten under the microscope. . . . The heart differs from the muscular tubes of the blood-vessels, inasmuch as, when it closes, all its diameters are shortened; whereas when it expands, all its diameters are elongated. . . . The fibres and the sarcous particles of the fibres are arranged vertically, transversely, and obliquely in continuous spirals. . . . The heart acts as a sucking and propelling organ, in virtue of its centripetal and centrifugal force. The heart has the power of forcibly expanding itself as it has of forcibly closing itself. . . . The centripetal and centrifugal wave movements pass through the sarcous elements of the different portions of the heart very much as the wind passes through leaves; its particles are stirred in rapid succession, but never at exactly the same instant; the heart is moving

This conception which perfectly corresponds to the facts of muscular action in the heart, the limbs, the hollow viscera, and the invertebrate animals, furnishes a solid base for the theory that the increased dilatation of the heart under the influence of quinine is due to an active stimulation of the diastole, and not to a commencing paralysis. According to Chirone, the same diastolic action is exerted by quinine upon the arterioles. The administration of quinine to a rabbit causes a marked hyperæmia of the ears, which grow hot, and after a while begin to pulsate. The same phenomenon is observed if the auricular nerve, carrying the vaso-motor fibres of the ear, is divided previously to the administration of the quinine. The passive dilatation of the blood-vessels which immediately follows upon this operation is much exceeded by the active dilatation under the influence of the quinine.

In a small rabbit to whom I gave hypodermically ten grains of quinine, the blood-vessels of the ears almost immediately dilated; in five minutes the animal had an epileptiform convulsion; five minutes later a second, which terminated fatally. In another rabbit the same sudden flushing of the ears occurred five minutes after a dose of 8 centigrammes, but in twenty minutes had began to subside. The animal died two hours later.

The action of quinine upon striped muscular fibre resembles that on the heart and arterial muscles in that it seems to finally depress the energy of contraction. Thus it is ranked by Brunton among the muscular depressants. I will not extend this paper unduly by analyzing this action of quinine, which only remotely bears upon the effects which here concern us. The accompanying tracings, however, taken from the gastrocnemius of a frog, excised and placed in Pfluger's myograph, may be interesting as showing the truth of Brunton's proposition. Traces vii to xii were taken with a stationary, the others with a revolving cylinder. It will be seen that the height of the contractions, as also the number obtainable before exhaustion, is decidedly less after the quinine.

When the revolving cylinder was used, the load on the

---

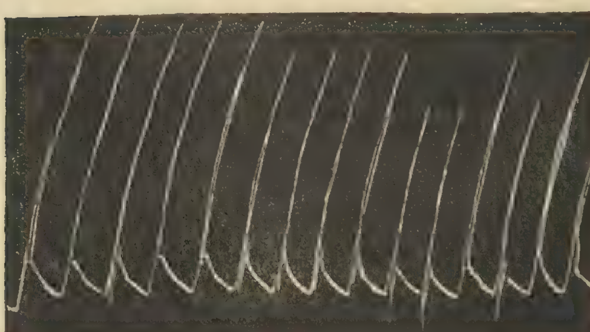
as a whole, but its particles are only moving at regular and stated intervals, the periods of repose, there is every reason to believe, greatly exceeding the periods of activity. . . . The position of rest does not correspond either to diastole or systole, but to a line midway between both."

muscle was increased from twenty to forty grammes; the distance of the Du-Bois-Reymond cylinder diminished from 210 to 150 millimetres. The excursions of the lever are enormously augmented both in the normal and in the quinine muscle and, about in the same proportion. Their maximum height in the first is $4\frac{1}{2}$ centimetres, in the second $3\frac{1}{2}$.

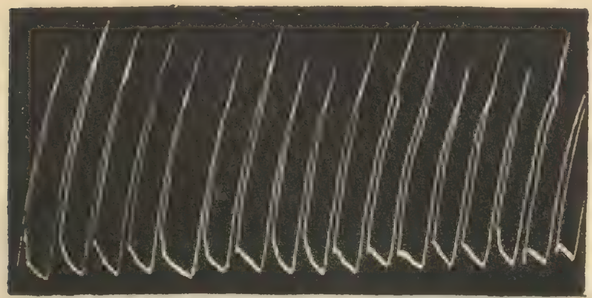
In the normal muscle were obtained sixty-three tracings of the first kind shown on the paper (traces xiii to xvii), before the energy of contraction is modified; the quinine muscle only gives fifty-one.

The normal muscle gave one hundred and forty-seven contractions before the shape of the tracing was markedly

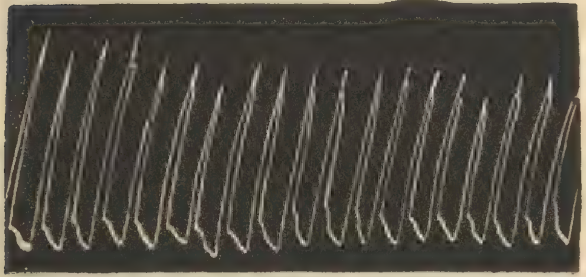
TRACE XIII.—Normal muscle; rotating cylinder; weight, 40 grammes; R. A., 150 millimetres; time, 3.56; shocks received automatically.



TRACE XIV.—Same continued.



TRACE XV.—Same continued.

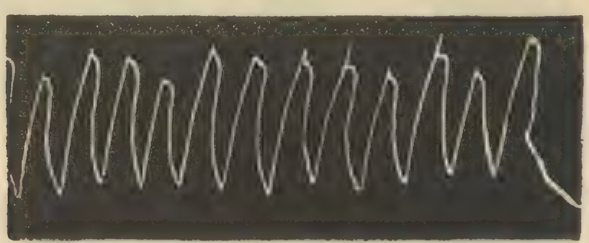


TRACE XVI.—Same continued.

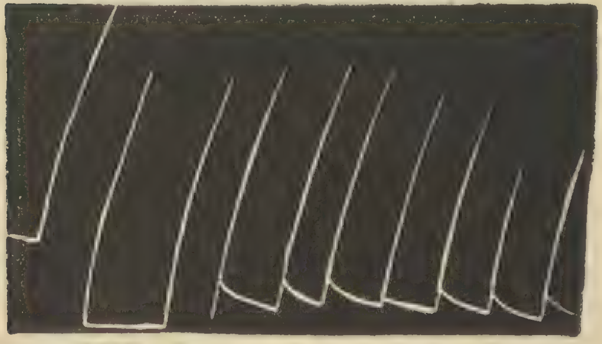
modified by prolongation of the descending stroke; but this occurred after the sixtieth contraction with the quinine muscle (traces xviii to xxiii). The descent of the lever corresponds to the relaxation or diastole of the muscle. In the quinine muscle this diastolic movement begins much earlier and becomes much more marked than in the normal muscle, though the irritability of the muscle, as shown by its response to the stimulus, seems to last as long with as without the quinine, and to be regained as thoroughly after repose. The slow and ample diastole should not, therefore, be attributed to paralysis, although the systolic—the contractile energy, usually so called—be diminished. But the diastole is directly increased in the voluntary as in the cardiac muscle. The application of these interesting observations to the theory of quinine in pneumonia is obvious and, it seems to me, important.

According to them, the immediate effect of the quinine would be an increased diastole of the heart, in virtue of which blood must be more energetically aspired into it and from the

lungs. Simultaneously, but correlatively, the systolic contraction is increased in energy, tending to drive the blood onward, in the pulmonary as in the systemic circulation. Thus, in a double way, an energetic influence is instituted calculated to dissipate congestion in the lung. This cardiac influence should

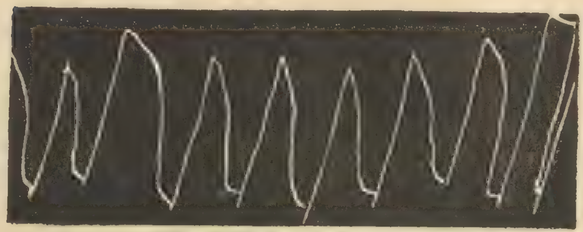


TRACE XVII.—Same continued at 4.03; total number contractions in 7 minutes, 147.

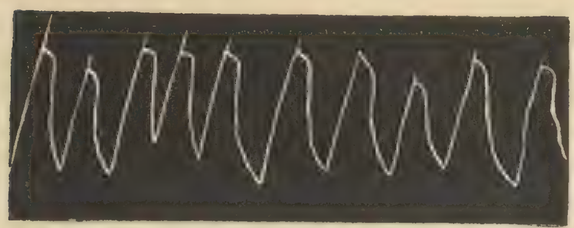


TRACE XVIII.—Gastrocnemius 30 minutes after 0.08 bisulphate of quinine; rotating cylinder; weight, 40 grammes; R. A., 150 millimetres.

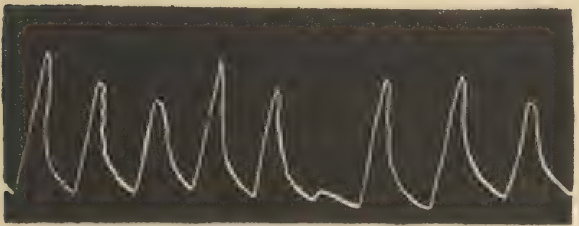
TRACE XIX.—Same continued.



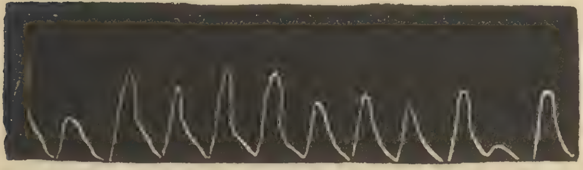
TRACE XX.—Same continued.



TRACE XXI.—Same continued.



TRACE XXII.—Same continued.



TRACE XXIII.—Same continued: total number contractions, 138.

be re-enforced by a similar increase of active diastole in the arterioles of the lungs themselves, due to a direct action of the quinine on their muscular fibre, and therefore independent of complex nerve influences. Without an increase in the energy with which blood may be drawn through and from the lung, an increased energy in the propelling force of the heart might

be disastrous rather than beneficial. From the point of view here developed it becomes clear why the best effects of quinine should be obtained with relatively moderate doses, rather than with larger ones. The latter are constantly threatening arrest of the heart from excess of diastole; to use the classical phrase consecrated by experience, they tend to depress the heart, and thus to antagonize the very benefit we hope to gain.

Again, it is clear that this beneficial effect can only be looked for upon the tissues which are the seat of congestion—the congestion peripheric to the foci of inflammation. But experience shows that, if this congestion can be limited, the characteristic morbid process will not extend. The same experience shows that exudations may remain in the lung without causing fever or being dangerous, except in so far as they are liable to caseation. The increased energy of the pulmonary circulation which may be effected by the quinine tends to arrest this danger, though, unfortunately, not always successfully. For the numerous considerations which have been alleged, it should follow that a direct antipyretic effect was not to be looked for in using quinine in pneumonia, however often high temperatures were observed to fall after its administration. It is both useless and dangerous to push the drug for this purpose; far better, if really excessive temperatures require symptomatic palliation, to effect that with small doses of antipyrine.

On the other hand, the absence of fever, while signs of consolidation persist in the lung, do not contraindicate quinine; on the contrary, quinine will often be followed by the most beneficial effects. These may be seen sometimes in cases of quite chronic pneumonia, or where, many weeks after asserted convalescence from a febrile pulmonary affection, a latent consolidation is discovered as the cause of gastric or nervous symptoms that had been considered quite inexplicable. Under the use of the quinine, not only these, but tubular breathing and percussion dullness may quite disappear.

I think the clinical facts which have been adduced in the beginning of this section help to bear out the conclusion that the *characteristic indication for the use of quinine in pneumonia is the dissipation of pulmonary congestion*. Theory and experiment indicate that this is primarily effected through an increase in the diastolic movement of both heart and arterioles. It is

not improbable that the same doses of quinine which do this *also re-inforce the medullary nerve-centers, and enable them to better resist vagus irritations*, and that thus, in a second way, the drug, though unable to really cure the disease, tends to limit it, and to arrest the tendency to death. But this large subject is beyond the scope of the present paper.

CASE OF PROBABLE TUMOR OF THE PONS.[1]

THE child whom I have the honor of bringing before the Society to-night has the following history.

Jennie Baer, aged ten years. The morbid symptoms are said to date from two and a half years ago, when the child was frightened by a trivial incident that she remembered with rather remarkable tenacity, and describes with lively interest. She was fastening the apron of a school friend, when the mother of the latter ran at her, uttering some injurious epithet. The child, the patient, ran away as fast as she could up several flights of stairs to her own room, and immediately began to tremble. Two days later, on going to school, this trembling was noticed by the teacher. It continued, and even increased for two weeks, at the end of which time the teacher advised removal from school. She has never returned, nor attempted any systematic exertion since, bodily or mental. A little while after the removal from school, the parents noticed that the child walked as if intoxicated. The father imitates this mode of walking by slightly swaying the body from side to side, leaning a little forward, dragging one leg after another without lifting them from the floor, but somewhat swinging each in a semicircle, as if to enlarge the base of support. It is the walk "en fanchant" of the French writers.

At this time, and on account of the trembling and inability to use the hands for writing or other fine work, the parents consulted Dr. Lilienthal, who diagnosed St. Vitus' dance, and treated the child accordingly, but without benefit. At this time, though there was said to be twitchings of the limbs, there was none of the muscles of the face. The head was, however, habitu-

[1] Read before the Neurological Society, December 2, 1888. Reprinted from *the Journal of Nervous and Mental Diseases*, 1889.

ally drawn a little to the left side, and the child exhibited the taciturnity common in chorea.

After this period, *i. e.*, a month or two from the debut of the accidents, the child seemed constantly tired, and usually spent the time lying down. She played very little.

At the end of about three months the child began to have trouble with walking, would frequently stumble, and occasionally fall. Could not go up and down stairs readily. It was at the same time that she began to grow very fat.

She was brought to my clinic at the Woman's Medical College, in May of this year, 1888, after two years' continuance of the above symptoms. She had then been suffering during several weeks from headache, continuous, diffused, but by no means violent. It may be noted at this point, that this headache disappeared after a duration of seven or eight weeks, and has not returned. There had been two attacks of vomiting, at an interval of several weeks. These have not been repeated.

When the child was seen by me in May, I was at once struck by her large fleshy size, the unusual development of the limbs, and especially the great size of her head. The measurements taken at the clinic have been lost; but a month later, Dr. Petersen kindly made a careful examination of the head for me, and, together with some other determinations, the following measurements were taken:

Circumference equals 57½ centim., or 22⅗ inches.

Naso-occipital line equals 38 centimetres, or 15 inches.

Binauricular line equals 38 centimetres, or 15 inches.

I omit for the moment the other calculations made by Dr. Petersen.

I could not ascertain whether the mother had been impressed by the large size of the child's head, or whether she had noticed that it increased. The habitual expression of the child's face was apathetic; the eyes heavy and lids drooping; but if spoken to, her face lighted up and she replied intelligently and with a smile.

There was no hyperæsthesia of the scalp, and percussion of the head elicited no pain. There was no disturbance of sensibility of any kind in any part of the face, trunk or limbs. No sign of paralysis of any cranial nerve. The movements of the upper extremities were normal, though there was a little clumsiness in the finer movements of the fingers, as in picking up a pin. There

seemed to be some paresis of the trunk muscles, at all events of the extensors of the spinal column; for the child avoided sitting upright, and continually leaned against the back of the chair for support. The conspicuous disturbance of function was in the lower extremities. The child could, though with some difficulty, rise from her chair and stand without support, but only for a minute or two, for she then fell forward. She could walk a few steps across the room, but then also would fall forward unless supported. During the attempt at walking, the body swayed a little from side to side, and the legs described the semicircular curves already mentioned. A month later it was noticed that the right ankle bent while walking, so that the foot turned inward. This was not perceived in May. At that time the child could walk up stairs more easily than on a level, because she supported herself by the bannisters. Faradaic sensibility and motility were intact for all muscles.

The knee-jerk was somewhat exaggerated on both sides.

The visceral functions were all normal; the appetite was even excessive. There was no unnatural drowsiness.

A re-examination was made of the case on October 13th.

The condition was found much aggravated in many respects. The circumference of the head has somewhat enlarged a little. Having been 57 centimetres, or $22\frac{3}{5}$ inches, when measured by Dr. Petersen in June, it is now $23\frac{1}{4}$ inches, a difference of $\frac{13}{20}$ of an inch. The naso-occipital and binauricular measurements remain the same.

The general appearance of the child is as already described; but she has grown still fatter, the girth of trunk, as measured by her dress which has burst out, being increased at least three inches.

The intelligence remains clear, notwithstanding the habitual listlessness. The child describes certain lines drawn on paper as vertical or horizontal, though she has not heard these terms since she left school two and a half years ago. She also relates the story of her original fright, and inquires with interest concerning the prognosis of her disease.

A new symptom now exists, in the intermittent divergent strabismus of the right eye. This is occasionally quite marked, but can always be overcome by voluntary effort, when converging the eyes to look at an object placed not too near. At a dis-

tance of two feet the nature and number of the objects are accurately distinguished; but when five lines are drawn on a paper, and the child looks closely at them, she calls them four. If she attempts to count the lines by placing her fingertip on them one after the other, the difficulty is increased. In six trials she invariably skipped the third line, and could not accurately touch the fifth. (They were drawn horizontally one above the other.) This difficulty depended partly on the inability to move the finger with precision; for it was agitated with slight choreiform movements during the intended act, which delayed its accomplishment, as is the case in sclerosis. But, in addition, there seemed to be some visual defect which interfered with the exact guidance of the finger and which rendered the patient unconscious of the fact that the finger had been placed above the line instead of on it. I could detect no diplopia; but I believe the lines seemed blurred to the child during the effort of convergence of vision upon very near and similar objects. This blurring she could not be made to describe.

There is no evidence of paralysis of the facial. The tongue protrudes perfectly straight. The uvula is slightly deviated to the right side. The tonsils are much enlarged.

Thus the only morbid symptoms in the sphere of the cranial nerves, apart from the optic, are: 1st, the intermittent irritation of the right abducens, as indicated by the intermittent divergent squint; 2d, the difficulty of converging the eyes upon near objects, so as to see them without blurring—difficulty apparently due to the relative or positive weakness of the right internal rectus.

The ophthalmoscopic examination was made later.

Upper Extremities.—The arms are very large; the forearms do not seem to be disproportionate to the age of the child. All movements of the arms and forearms can be executed without perceptible difficulty. Difficulty first appears when the child attempts to carry a glass of water to her mouth. Left to herself, she takes this in both hands, as if distrustful of her ability to hold it in one, although, on being tested, she can do so, and even, though unsteadily, carry it to her lips. When the glass is held by both hands against the mouth, she drinks readily until it is half emptied, but then seems unable to tip the glass at a greater angle in order to drain it. This movement necessitates the inclination

of the hands on the radial border of the forearms, action effected by the supinator longus, and the longer and shorter radial extensors of the wrist. When there is no weight in the hand, the child can flex and deflect the hand on the radial border of the wrist; the difficulty only occurs when the hand is carrying a weight and for a special purpose. There is therefore no actual paralysis of the muscles, but paresis and diminished power of co-ordination.

A similar difficulty of co-ordination is shown in the fingers when the child tries to write. Before the hand reaches the pen it is agitated with slight choreiform or ataxic movements, as is also the case when she picks up a pin. She cannot go straight to the object. She places her head to one side, the right, in order to guide her hand in writing. She then forms the letters very imperfectly, and cannot write them in a straight line. The word (she resigns the attempt after the first word) always runs obliquely off the page, running up sharply from left to right. It is to be remembered, in connection with this test, that the child's education ceased when she was only eight years old, and before she had really learned how to write. Apart from efforts at functional co-ordination, all movements of the fingers can be performed voluntarily and without distinctly perceptible diminution of force. The grasp of the hand is normal for a child of her age. Sensibility is intact for the whole upper extremity.

The faradaic contractility and sensibility are intact.

Trunk.—The child cannot support herself upright, even in a sitting position, for more than a minute or two, but leans back in the chair, showing paresis of the spinal extensor muscles.

The thighs and legs are noticeable for their large size, although the apparent muscular enlargement of the four extremities is not as striking as is that of the thorax and abdomen. While sitting, the child can extend the legs on the thighs perfectly, but not vigorously, and can flex, extend, and adduct the feet and toes. Abduction is extremely feeble on both sides. Nevertheless, she is unable to support herself standing for more than a half minute, and cannot now walk at all unless supported. With support she walks, dragging and to some extent swinging round the legs, while both ankles bend under her, bringing the feet into exaggerated varus. Thus there has been marked deterioration in the functional power of the limbs since May, and

even since June, when the special weakness of the peroneal was first observed, and then only on the right side.

There is no retraction of the gastrocnemii muscles. The child has never walked on the toes, nor never exhibited the special phenomenon of pushing back the thighs or of climbing up them in rising. Nor was there ever lordosis, but the trunk always fell forward.

In May, as already stated, the child did pull herself up stairs by the aid of her arms. Also, while she was yet able to walk unsupported, the body did sway from one side to the other, as if from alternate inclination of the pelvis, and the legs spread outwards to enlarge the base of support. These symptoms have now disappeared, as the paresis has increased so far that the child cannot even attempt to walk unsupported.

The absence of retraction in the gastrocnemii is paralleled by similar conditions throughout the limbs, where neither contracture nor deformity exists. The sensibility is intact. All the muscles contract to the faradaic current, but the peroneal muscles demand a stronger current than the other. Thus the quadriceps extensors contract at 300 millimetres of the induction coil, the peroneal not under 260. The knee-jerk is moderately exaggerated. No ankle clonus can be obtained.

The limbs are constantly cold, subjectively and to palpation.

In addition to the foregoing symptoms may be noted:

That the child suffers no pain in any part of the body, nor has ever done so except for a moderate headache in last spring.

There is neither nausea, vomiting, nor anorexia, but, on the contrary, an excessive appetite, bordering on bulimia. There is occasional sighing respiration.

There is no constipation, but a condition of the bowels approaching incontinence, inasmuch as the child is unable to restrain the impulse for evacuation when this makes itself felt; and the impulse always occurs after any mental excitement, as if she has been laughing. The control of the bladder is similarly imperfect.

On the 28th of October Dr. C. S. Bull kindly made an ophthalmoscopic examination of the child. He found choked disk in both eyes, outlines much blurred, veins swollen and tortuous, arteries narrowed and in some places disappeared; process beginning to recede, but leaving an atrophy of nerves, which is now

incipient, but tending to increase. Acuity of vision much diminished, only $\frac{10}{100}$. Spasm of right abducens. He diagnosed a descending inflammatory process of the optic nerve.

In 1887 Dr. Hun, of Albany, presented the following case, completed by the autopsy, to the American Neurological Association:

Gliomatous hypertrophy of the pons.

Female, æt. 6; father died a little more than a year after she was born, with symptoms of melancholia and dementia.

For several years left leg has tired easily; left foot shows a tendency to turn in; wore rubber straps; otherwise excellent health until two months ago, when attack of croup and cough. Pain felt in head with each cough. Three weeks before coming under observation the patient began to walk badly, and seemed to have trouble in balancing herself while walking. She had an excessive appetite and vomited a little at times.

When first seen the patient was a well-nourished, intelligent girl, but with a vacant expression. Her speech was drawling. Her head was drawn towards the right shoulder most of the time, especially if she made any exertion. She stood with her feet wide apart, and was careful not to lose her balance. In walking, her right leg was more rigid than her left, so that she took freer and longer steps with her left leg, and therefore in walking tended to go in a circle, turning always to the right. Her walk resembled that of a drunken person. The movements of the arms, especially of the right, were very awkward, but she held them in no fixed position.

There was no disturbance of sensibility in any part of the face, body, or extremities, and she recognized objects placed in her hands when her eyes were shut.

The plantar reflexes were normal. The knee-jerk was exaggerated, especially on the right side. There was no ankle clonus.

On ophthalmoscopic examination, well marked optic neuritis was found in both eyes. Urine contained neither albumen nor sugar.

The patient was first seen on the 17th of April, and from this time the symptoms rapidly increase, passing through gradations I need not here describe, until a fatal termination was reached on the 13th of June.

At the autopsy, the pons varolii was found enlarged to three or four times its normal size, and on section, it was found to have been replaced by a tumor, apparently a glioma, which so well preserved the normal appearance of the part that it looked like a greatly hypertrophied pons.

On microscopical examination, the nervous elements of the pons were found to be encroached upon, infiltrated, and in some places destroyed by a great accumulation of small cells. The proliferation of cells was not limited to the pons, but extended throughout the crura cerebri and medulla, and especially in the roof of the aqueductus Sylvii. The prevailing character of the cells was spindle, but there was a considerable number of spheroidal cells, both large and small.

The bones of the skull were thin. The subarachnoid fluid increased. The cerebral convolutions flattened, and the cerebral substance very œdematous. Lateral ventricles greatly dilated, and filled with fluid of normal appearance.

Dr. Hun ascribes the inco-ordination of movements to pressure upon the transverse fibres of the pons, and the origin of the crura cerebelli; the absence of absolute paralysis to the fact that the nerve elements were compressed but not destroyed by the infiltration. He explains the absence of sensory disturbance by a greater resistance of sensory function, even when sensory fibres are submitted to the same pressure as motor fibres. He notes that the tumor in its growth produced no symptoms of irritation, that there were no convulsions, and but little headache.

In all these circumstances, the case closely resembles the one I now present. Other points of resemblance are the long duration of slight muscular inco-ordination, and even paresis of the lower extremities, without other symptoms, up to a certain date; then the rapid deterioration in a few months; the fact that the most marked paresis was shown by the turning in of the right ankle; the exaggeration of the knee-jerk, the other reflexes remaining normal; the vacant expression of face, and apathetic appearance, although the memory and intelligence were entirely preserved; the indistinctness of articulation; the optic neuritis, the slight and transitory squint, which in Dr. Hun's case was convergent, in this one divergent and unilateral, without paralysis of any ocular muscle; finally, the excessive appetite and excellent preservation of nutrition. The nutrition in our case is even

exaggerated, so as at one time to have suggested a general lipomatosis.[1]

The optic neuritis is, as well known, a most important indication of intra cerebral tumor; and indeed I did not permit myself to make a positive diagnosis until the existence of this symptom had been established. Bramwell enumerates the following diseases other than mental in which double optic neuritis may occur. It is nearly always present in lead encephalopathy; it is not uncommon in meningitis and cerebral abscess; and it may occur in Bright's disease. It has occasionally been associated with uterine derangements, hypermetropia and anæmia, which, when complicated with hysterical symptoms, may be easily mistaken for cases of intra cranial tumor.

In our case all the foregoing conditions may be readily excluded.

Stephen Mackenzie has published in the second volume of *Brain*, a case where an optic neuritis depended upon a diffused cerebritis, resulting in general atrophy of the brain, and an inflammation extending down the optic nerve. The symptoms of this remarkable case, which has often been quoted, are chiefly to be referred to the cortex of the brain, and in no wise resembled those of this child.

When I first saw the patient, the unusually large size of the head, the apathetic expression of face, the moderate degree of motor disturbance, the diffused nature of the symptoms, led me to suspect an hypertrophy of the brain, due to diffuse lobar sclerosis. "In such cases," observes Gowers, "the symptoms have been very similar to those of cerebral tumor, headache, vomiting, local palsy, convulsions."[2]

"The symptoms," observes Schmidt,[3] "of diffused sclerosis of the brain are gradually increasing muscular weakness, manifesting itself especially in the lower extremities, giving rise to an unsteady, stumbling gait and frequent falling. There are also epileptic spasms, constant or intermittent attacks of headache, vertigo, tinnitus aurium, photophobia, dimness of sight with dilatation of

[1] A case of tumor of the pons and medulla in a child of two years, related by Hobson, (*Brain*, Vol. IV., p. 531), differed greatly from ours, especially in the number of cerebral nerves involved.

[2] *Diseases nervous system.*

[3] Pepper's *Archives of Medicine.*

the pupil, blunting general sensation without anæsthesia. The skull may be enlarged and thinned."

So Richardière[1] affirms: "Convulsions and attacks of tremor and muscular rigidity are never absent in cases of diffused sclerosis." Both these symptoms have been entirely lacking in the case of Jennie Baer. On this account the hypothesis of the diffuse sclerosis was finally rejected.

To sum up: the reasons for diagnosing an intracranial tumor in the case of the child before you, are: 1st. A diffused motor disturbance, beginning as inco-ordination and difficulty of equilibrium, increasing at first slowly, then with sudden rapidity to such paresis of the lower extremities as renders station impossible; of the trunk muscles, as renders upright sitting difficult; of the upper extremities, as interferes with the more delicate movements of the hands. 2d. Preservation of faradaic contractility, absence of nutritive lesion of the paretic muscles. 3d. Gradual enlargement of the head. 4th. Moderate apathy of expression and dulling of intelligence. 5th. Spasm of right external rectus. 6th. Double optic neuritis, commencing atrophy of the optic nerves, marked diminution of visual acuity.

The circumstances which indicate a localization of the tumor in the pons are: 1st. The absence of convulsions, a negative fact of great importance, and frequently observed in slowly infiltrating tumors of the pons. 2d. Absence of marked or definite symptoms, and of monoplegic spasm or paralysis, tend to exclude tumors of the cortex. 3d. Absence of hemiplegia excludes the basal ganglia. 4th. The same, with absence of symptoms in the sphere of the motor oculi, excludes tumors of the crura or base of the brain. 5th. Absence of headache, nystagmus and vomiting, and the development of motor paralysis in addition to the original motor inco-ordination, excludes, I think, the cerebellum. 6th. The general march of the symptoms, the bilateral character of the paresis, the inco-ordination, even the absence of anæsthesia or pain, are precisely what have been observed in slow growing tumors of the pons. In seven out of thirty cases of pontine tumor tabulated by Bernhardt, no disturbance of sensibility existed but headache, and even this failed in two cases, as also in that of Dr. Hun. The irritation of the right abducens, the only cranial nerve at present affected except

[1] *Scleroses encephaliques primitives de l'enfrance.* Paris, 1885.

the optic, is in accordance with a localization of disease in the pons. The excessive appetite of the child is a symptom probably to be referred to the medulla.

Characteristic symptoms of pontine tumors which are as yet absent, are: 1st. Alternate paralysis of face and limbs. 2d. Paralysis of the hypoglossus, or of any cranial nerve, other than the optic. 3d. Marked difficulty of deglution or articulation. Some defect of the latter function is, however, noticeable, and there is occasionally sighing respiration. The tumor could hardly be situated in the upper region of the pons, or there would be motor oculi symptoms, or others referring to the corpora quadrigemina; while at the lowest portion of the pons, the spinal accessory nucleus or the hypoglossal nerve should be involved. We may infer that the growth is situated about the middle region, that it is bilateral, that it is below and anterior to the nucleus of the fifth and anterior to the nucleus of the seventh nerve. That on this account the lower extremities are paralyzed before the upper, and the seventh has so far escaped. We must further infer that the lesion consists of a very gradual infiltration of elements, gliomatous or sarcomatous into the nerve tissue, and is not a sharply defined new growth, forcibly compressing any localized bundles of nerve fibres.

One other hypothesis remains,[1] namely that there is a tumor of the cerebellum which presses on the pons. The reasons which militate against this hypothesis are, that there have never existed any characteristic symptoms of cerebellar tumor, except the tottering walk and loss of equilibration. But these symptoms are also observed in the infiltrations of the pons which affect the transverse crura cerebelli.

In the four cases of cerebellar disease reported by Dr. Seguin,[2] the following symptoms, positive and negative, were observed, differing from the history of Jenny Baer.

CASE I.—*Positive:* Headache, nystagmus, convulsion. *Negative*: Absence paralysis until after hæmorrhagic seizure two months before death.

CASE II.—*Positive:* Vomiting, violent headache, convulsions.

[1] This was strongly urged by Dr. Sachs at the meeting of the Neurological Society when the above communication was read.

[2] *Journal Nervous and Mental Disease*, vol. xiv., April, 1887.

Case III.—Diffused headache, vomiting, double exophthalmus, absence paralysis.

Case IV.—Repeated nausea and vomiting, severe occipital headache, absence paralysis.

The paralysis in the case of Jenny Baer is not indeed complete but it is sufficient in the lower extremities to render standing quite impossible which is more extensive than is ever the case with purely cerebellar tumors.

THE PRACTICAL STUDY OF BIOLOGY.[1]

Remarks addressed to the Massachusetts Medical Society at its Annual Dinner June 12, 1889.

The 108th Annual Meeting of The Massachusetts Medical Society, Boston, June 11–12, 1889.

Doctor James B. Chadwick presiding "where the white spread tables made a handsome picture and where Baldwin's Orchestra had already begun to enliven the occasion." Doctor Chadwick introduced Doctor Mary Putnam Jacobi as "one whose life had been spent in pursuing the most abstruse subjects in medical science."

Doctor Jacobi spoke on the practical study of biology.

MR. CHAIRMAN,

In accepting the very great honor of an invitation to be a guest at the annual meeting of the Massachusetts State Medical Society, I did not expect to be called upon to speak before it. But since, Sir, you have chosen to still further honor me by calling upon me, it would be churlish to refuse.

In a meeting like this, I presume it is intended that each person present shall bring forward the thought or thoughts that may have especially preoccupied him or her during the year that has gone by. Now, one of the subjects which has especially preoccupied me is one that I think must interest everybody who is either a student or a physician or a director of medical education. I refer to the question of the practical study of biology, in its threefold aspect of normal physiology, of pathology, and of elementary therapeutics.

There is a strange idea current among the laity, and even

[1] Reprinted from the *Boston Medical and Surgical Journal*, 1889, Vol. 120, page 631.

among physicians, that the study of physiology by means of practical experiments and demonstrations on the living subject is only necessary, and indeed only permissible for purposes of occasional original research. You know that this is the view taken by so august, yet in the premises so incompetent a body as the English Parliament, which has assumed the right and duty of actually forbidding physiological experimentation for any other purpose. But in reality the necessity is not occasional, but permanent, and coextensive with the education of every medical student. For what are the facts of the case? We undertake to prepare ourselves and others for the most profound and subtle and difficult of all sciences, the Science of Life; we undertake this, not for the purpose of irresponsible contemplation, but with the avowed intention of practically intervening among the phenomena of life, of regulating disturbances in the mechanisms of living organisms, of bringing normal order out of what may have become the wildest disorder and confusion. Now, how is it possible to do this unless the mind has first become thoroughly and personally acquainted with the normal order? Students in other natural sciences than animal physiology know perfectly well that listening to a didactic lecture or conning the pages of a text-book are entirely insufficient means to bring the mind into fertile contact with nature. Such contact cannot be obtained second-hand, but only by those who, as Claude Bernard says, have in the hospital, the amphitheatre, and laboratory, with their own hands stirred the soil fœtid and palpitating with life. The difference is enormous between the person whose knowledge of physiological phenomena is summed up in a list of verbal statements, and the other whose mind has become saturated with vivid conceptions of vital facts, based upon multiple experience of them. No one who has not tried it, knows how indefinitely both intellect and senses gain in delicacy and subtlety when they have become habituated by practical intercourse with the endless intricacies of nature. And it is the peculiarity of medical work that the necessity for such mental subtlety is not confined to the few, to the *élite*, to the college professors who, very likely, have withdrawn from the practice of medicine. The poorest sick person in the hospital, the most tedious invalid of the private *clientèle,* serve to illustrate all the mysteries of the science, and demand on the part of their doctor

the ability to cope with its deepest and most difficult problems. The physician whose thought is sufficiently elevated, and whose imagination is sufficiently keen, confronts this commonplace sick human body as an antique priest may have stood before the veiled mysteries of Iris. It is his privilege, and to an increasing extent, to draw aside the veil of the surface, to plunge his eye into the depths of the organs and tissues as they are actually at work, to follow in imagination the innumerable streams of vital actions which are eddying and swirling in every direction, and to try, out of the dizzying maze, to construct a truthful chart of the vital conditions and tendencies of the organism. If he cannot do this, or attempt to do it, if he has not been previously trained to a profound feeling for the complexity and intricacy of vital processes, he is liable to lay heavy and clumsy hands upon them, and to estimate them according to theories both coarse and crude. When he gives a medicine he simply "exhibits" it. He has no distinct conception of a train of vital events, among which he is to insert some new and deliberately contrived conditions, by means of which the direction of the whole series may be modified. This is the ideal formula for physiological therapeutics. It is far enough from being realized or realizable to-day; but it seems to me no one can doubt that it is ultimately destined to supplant the rule of traditional "Secundum artem," with which at present we must too often be content.

It would be indeed the merest pedantry to attempt to base existing therapeutics exclusively upon existing physiological knowledge. No practising physician can or would follow Herman's classification of remedies, when he places first the utterly useless oxide of carbon because we know all about it, and consigns to a limbo of doubt opium and quinine as drugs of which we know little or nothing. Whether we know or not, we are compelled to use them. But it is absurd to say that we know nothing because we do not know analytically. We do possess an accumulated amount of experience in regard to these priceless remedies which is of immense and scientific value, although the ultimate reasons for their action have not yet been demonstrated. But on this very account, and because an exact and quantitative formula cannot be given for the action of drugs, we are obliged to intrust their handling to the acquired tact of long practice. Now tact, mental or physical, can only be acquired by incessant

and varied experience in the practical operations requiring the exercise of tact. How can be acquired the mental tact needed to unravel the complexity of vital phenomena as they unravel before us in the history of every sick person? how can be learned the tact needed to undertake the direction of these phenomena, except by previously prolonged study of vital phenomena in the simpler animal body free from disease?

If it be admitted that the dead body must be dissected, as was still practically denied less than a hundred years ago; if it allowed that the lesions left by disease must be studied in autopsies, which are still often refused in obedience to the grossest superstitions; if it be conceded that medical students must have some clinical experience with the sick before they are allowed to graduate, concession which still often remains a paper right,—it equally follows that the same persons must study living organisms in the only way in which they can be studied, by analyzing, dissecting, and handling them in the process of life. There is no need now of wading through the horrors of physical pain amidst which Magendie laid the foundations of modern science. The use of anæsthetics obviates the objections on the score of cruelty for this as for other forms of surgical operation; and it is certainly one of the most extraordinary demonstrations of modern science that life can be so dissected and dismembered and yet persist.

The kernel of the question lies just here. Knowledge of the living organism, for practical purposes, must be obtained practically. Indeed, no knowledge is ever obtained in any other way, but only a verbal imitation of knowledge. The physician who has not learned to adjust himself to the intricate delicacies and fragilities of living organisms by laboratory study, is condemned to take his first lessons in dealing with life upon human beings. He is compelled, therefore, to experiment upon subjects who are often, if not always, far more valuable than the frogs and rabbits of the laboratory.

Sooner or later, if he is to be successful, his whole mind must have become modified in that mysterious manner in which the minds of students of nature do become changed, as they seem insensibly to blend with the phenomena they can profoundly contemplate. But the question is, shall this necessary training come sooner, or later? Shall it come economically, or with

tremendous though carefully concealed expenditure of human life? Whether we will it or no, all practising physicians are constantly and professionally engaged in physiological experimentation, are trying the most audacious of experiments,—trying, namely, to deliberately modify the course of human life. Is it not evident, therefore, that we should strive to the utmost to obtain the most real and profound visions of life before we venture on our attempts at interference?

I should like, Mr. Chairman, to mention an incident that occurred to myself in the course of a very simple laboratory experiment. I was examining the circulation of a frog's lung by means of the Holmgren apparatus. I happened to so focus my lens that all the outlines of the capillaries and blood corpuscles disappeared, leaving visible only the spaces between the epithelial cells. Nevertheless there remained a vision of the streaming movement of the invisible blood through the ramified spaces. The streaming was so rapid, so energetic, so ceaseless, it seemed as if it were pure motion or force divorced from the accidents of matter. The microscopic shred of tissue from the insignificant animal seemed for the moment to give a glimpse of a mighty vision of endless life, streaming with infinite energy into the minutest particles of an infinite universe. The impression was indescribably powerful. Since then I have confronted new students with this same impression, for the purpose of throwing open at once the horizons towards which they were henceforth to keep their eyes directed. And this is what it seems to me all students in medicine should learn to do.

HYSTERICAL FEVER[1]

Read at the Neurological Section of the Academy of Medicine, April, 1890.

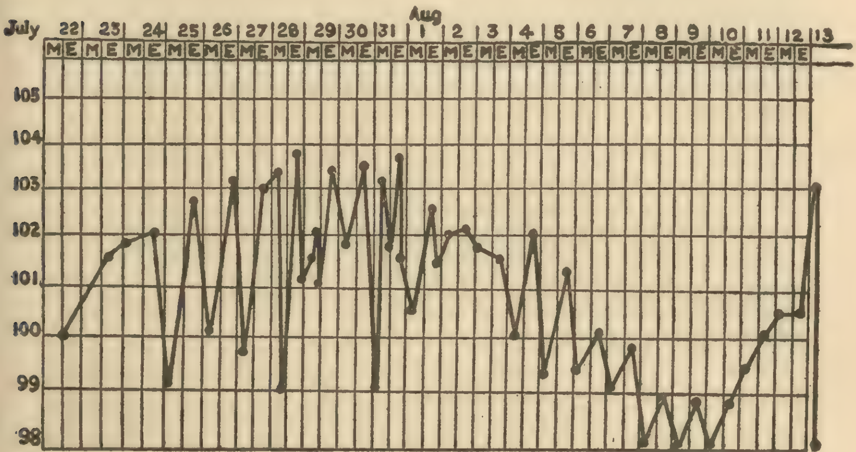
CASE M. M.—The illness to be described began on July 22d last, 1889, when the patient entered the New York Infirmary. But in the preceding year, winter of 1887–1888, she had suffered from a succession of disorders, to which reference must be made on account of their bearing on the illness in question.

In December, 1887, the patient was seized with a pain in the chest, unaccompanied by fever, but which was diagnosed pleurisy by the first physician consulted. His diagnosis had greatly alarmed the patient. As, however, my own examination of the chest failed to discover any physical sign of pleurisy, I interpreted the pain as a pleurodynia. After this diagnosis, the pain rapidly subsided; but a paresis of the bladder, which had already showed itself, deepened to a complete paralysis and retention of urine. Catheterism was performed for some time, but the trouble finally yielded to strychnine and local faradization. There appeared severe pain in the left ovarian region, attended with fever. The temperature rose and fell irregularly through the day, occasionally going as high as 103°, more often reaching no higher maximum than 102°. Physical examination of the pelvic organs failed to discover any objective sign of local imflammation, and the disease finally subsided. Before the patient had left her room, however, she was attacked with a severe catarrhal sore throat, attended with abundant diffuse mucous exudation, but not truly diphtheritic. This was in March. After recovery and resumption of ordinary occupations, the patient became subject to intermenstrual

[1] Reprinted from the *Journal of Nervous and Mental Diseases*, 1890.

metrorrhagia, for which no uterine cause could be ascertained, and which was referred to one of the obscure forms of functional ovarian irritation. During the summer of the same year (1888), the patient suffered from a bilateral partial paralysis of the lower extremities, especially affecting the peroneal muscles. She remained able to move her limbs in bed, but was quite unable to stand or walk. She recovered this power, however, when provided with braces which supported the ankles and reaching to the knees. She then went to the seashore, and for two or three months was perfectly well. On returning to the city and becoming involved in much mental worry and anxiety, her physical troubles returned. There were first, attacks of retention of urine and metrorrhagia; then almost entire inability to use the eyes in reading, which a competent oculist explained by simultaneous paresis of several external ocular muscles. He referred this, moreover to an attack of diphtheria which had been experienced five years before; but I think this was improbable, as, until the period which I have just described, the use of the eyes had been attended by no difficulty. It seems to me that the paresis was of an hysterical nature and analogous to that of the peroneal muscles, which had deprived the patient for a time of the power of walking. This opinion was subsequently also expressed by Dr. Putnam, of Boston. The ocular defect persisted through the winter. Twice during this time the patient was seized with an attack of severe pain in the abdomen, which, after lasting twenty-four hours, at once lost its acuity and rapidly subsided, when I had assured her with great positiveness that she did not have peritonitis. Once, after receipt of an agitating letter, she became apparently delirious and unable to speak for twelve hours. This was immediately followed by an intense dysphagia, overcome at last by a combination of moral force and local faradization. In the following summer, 1889, the patient's health was considerably improved. She engaged in some occupation involving considerable fatigue—I think teaching in a public night-school—and at once began to lose ground again. Early in July, during the second day of a menstrual period, she accompanied a friend on an excursion to Bedloe's Island, and climbed the stairs within the statue. The menstrual flow was immediately arrested, and severe pain appeared in the left

ovarian region. On the 22d of July she was admitted to the New York Infirmary, and on the 23d had a temperature of 102°, and the next day of 103°. The fever persisted at about this range till the 13th of August, when, after two days of normal temperature, it rose once more to 103°, and thence fell to a range between 98° and 101.5°, which it maintained till the middle of September. During this time I did not see the patient, as I was absent from



the city. The physicians in charge had only a very slight acquaintance with the previous history of the patient, and she herself gave an imperfect and rather misleading account of her series of illness. On account of the fever, the abdominal pain was explained by some focus of parametritis, but it was noted that the pelvic examination—made, it is true, with reserve on account of the acuity of the accidents—always failed to detect any evidence of inflammatory exudation.

On my return in the middle of September, the patient was in about the same condition as at the beginning of the attack, and quite the same as in the middle of August, after the fever had fallen to a low grade. Upon hearing the history and combining it with that of the many and varied attacks which I had previously and minutely observed, I ventured to express the positive opinion that on this occasion also no really inflammatory process had ever existed, but that the accidents were nervous, and initiated by an ovarian irritation, the latter due to

the arrest of menstruation by an unwonted physical exertion which involved the nerves of the lower extremities, *i.e.*, of the lumbar plexus, which also innervates, to a great extent, the ovary. It seemed probable that the menstrual arrest had left a congestion of the ovarian cortex, or even that minute hemorrhages had occurred there. The patient was put under ether, and a most thorough pelvic examination made, both by myself and by Dr. Cushier, with a completely negative result. Dr. Cushier admitted that the entire absence of any trace of exudation at this time, though some irregular low fever persisted and the abdominal pain was as severe as ever, rendered it altogether improbable that a parametritis had ever occurred.

With the concurrence of Drs. Cushier and Kilham, therefore, I positively assured the patient that she had no pelvic inflammation, that the attack was of the same nature as the others in which I had previously attended her; that she could safely get up from bed as soon as she pleased; and that a few applications of galvanism to the abdomen over the seat of the pain would rapidly dissipate it.

The applications were, in fact, made with the positive electrode over the ovarian region of the abdomen, the negative over the lumbar spine. Each application entirely removed the pain for many hours. But it seems probable that the moral effect of the diagnosis was quite as important, so rapidly did the patient change her attitude and so soon was she able to get out of the bed on which she had been lying for two months. In a week she was walking about; in ten days was entirely free from pain. The temperature remained normal from the day of the examination under ether.

Before the modern researches upon fever as the result of poisonous material circulating in the blood, the conception of a purely "nervous fever" was an entirely familiar one. Indeed the abdominal typhus, which is now recognized as a typical example of infectious disease, was considered, not so very long ago, as a "nervous fever," and liable to be produced by causes which greatly fatigued or exhausted the nervous system.

The well-known urethral fever was an admitted case of a purely nervous fever of reflex origin. "Febrile movements" of all kinds were easily explained by varying functional irritations of the nervous system, among which were not reckoned irrita-

ments conveyed to nerve centres in the blood nourishing them. Indeed, even the fever of inflammation was referred to the peripheric irritation of the nerves of the inflamed tissues; and not until much later was it suggested that some *materies morbi* was carried from the focus of inflammation to the central nervous system.

To-day, however, the point of view has so radically changed. that it is easy to forget that all the modern explanations of fever simply increase the list of irritaments to which the pyrogenic apparatus of the nervous system is susceptible. Although there be, as there undoubtedly is, increased production of heat during fever, it is established that this would not cause a rise of body temperature unless the elimination of heat were simultaneously deranged absolutely or relatively. But this derangement in the elimination of heat depends upon disorder of the heat-regulating apparatus of medullary and cerebral centres, which thus react to the influence of the chemical poisons generated by inflammation or infection. There is, therefore, no essential contradiction between the new and old views about fever. An exclusively nervous cause is always plausible, because the proximate cause of increased body heat is always to be sought in the nervous system.

Before the thermometer was supposed to enable us to differentiate with precision between inflammatory and non-inflammatory pain, the liability of hysteria to simulate inflammations, and especially those of the abdominal cavity, was one of the well-worn themes of text-books. "Hysterical Peritonitis" is a classical chapter in every dissertation on hysteria, and in every guide to differential diagnosis in abdominal disease. But I think that to-day—and the case I have related shows it—we are liable sometimes to be misled by an habitual, though legitimate, reliance on the thermometer as a means of differentiation. It is easy to decide in the absence of fever that pelvic pain must depend upon some other cause than inflammation; and in the great majority of cases this conclusion is confirmed by the absence of all physical signs of exudation. Yet Dr. Thomas and some other gynecologists declare that an extensive pelvic exudation may be formed, and with considerable rapidity, without the slightest rise of temperature ever being produced. However this may be—and I confess never to have myself seen

the statement proved—the two attacks of pseudo-parametritis attended by fever, which were sustained by the highly hysterical patient under discussion this evening, serve to illustrate the converse proposition, namely, that a rise of temperature may occur under circumstances strongly suggestive of pelvic inflammation and yet all positive proof of true inflammation be entirely lacking.

Hysterical fever has lately received much attention from both English and French physicians. In 1883 Pinard wrote a thesis on the pseudo-fever of hysterics, in which he claimed to show that hysterical fever did not really exist:—that is, in the cases described: either no thermometrical observation had been taken, or the thermometer registered a temperature not above 38° C., while often the temperature remained normal. The pseudo-fever consisted, therefore, in an assemblage of symptoms which simulated fever, but were not truly febrile. Among these was conspicuous the acceleration of the pulse, phenomenon essentially analogous to the tachycardia of exophthalmic goitre. The patients often had subjective sensations of heat, also severe headache and coated tongue. This condition was not unfrequently regularly paroxysmal, so as to simulate attacks of malarial fever, but was entirely uncontrolled by quinine.

In a more recent thesis, passed by Henri Fabre in 1888, the existence of a true fever, and even hyperpyrexia of really hysterical origin, is, however, formally reasserted. Cases are related where such fever was accompanied by functional disturbance of various organs, so as to simulate respectively meningitis, peritonitis, or pneumonia. Intermittent fever and typhoid fever are also said to be simulated. The same assertion is made by an American physician, Bressler, in a communication to the *Medical Record*, for 1888. This writer relates no cases in detail, and I do not think that his diagnosis is absolutely proved by his descriptions.

"By hysterical fever," says Dr. Bressler, "I mean a perverted condition of the nervous system, occurring in a neurotic individual, attended by an elevated temperature, which may last from a few hours to several days, and is associated throughout its duration with symptoms of an hysterical character." "This fever," continues the writer, "generally begins with

symptoms simulating a mild intermittent—chilliness, loss of appetite, constipation, or occasional diarrhœa; tongue coated, headache, general malaise, rise of temperature, face flushed generally, or in a circumscribed spot on the cheeks, eyes clear and brilliant, mind bright, comprehension quickened. There is general muscular and cutaneous hyperæsthesia. The special senses are more acute; there is no true delirium. The stomach is excessively irritable, and vomiting very persistent. The abdomen is extremely sensitive to pressure, and peritonitis may be simulated, but may be excluded by the fluctuating character of the pains, the absence of tympanitis, and the development of ovarian pain under pressure. The temperature varies from 101° to 105° F., and the maximum is reached early in the attack."

In the *Transactions of the London Clinical Society*, Dr. Hale White related the following case: A girl of eighteen was admitted to the ward, on the 10th of August, for a febrile attack, which lasted four days, and then subsided. On September 8th she was suddenly taken ill with a severe pain in the left side, and was readmitted to the hospital the next day. The patient could hardly walk, and was somewhat incoherent in speech. Within the course of twelve hours the pain was located in four different places—the left iliac region, the epigastrium, the lumbar region, the splenic region. The attention of the patient was easily diverted by conversation, and she then permitted considerable pressure over the seat of the pain. The temperature was at first 103°; on September 10th, after a chill, rose to 105°, to fall in the evening to 99°. On the 11th, at 6 A.M., the temperature was 98.6°, at 6 P.M. 104°; September 12th the temperature did not rise till evening, when it was 102° at 6 and 98.8° at 10.

In the analysis of the case Dr. White excluded all other causes of either the pain or the fever except hysteria. But it is noticeable that the patient vomited on two successive days, and during the previous brief illness in August there had also been symptoms of a gastro-duodenal catarrh. It seems to me that such an organic condition really existed, and was the immediate cause of the neurotic condition upon which the wandering pains, and markedly irregular fever, directly depended.

Dr. White remarks that, although several cases of hys-

terical pyrexia have lately been recorded, much skepticism has been expressed in regard to it. Among these recorded cases is one by Clemrow, in the *Medical Press and Circular*, of 1887. A laundrymaid, of twenty-three, was admitted to the Edinburgh Royal Infirmary, October 22d, with dizziness, pain in the left side, and a purpuric rash over the lower extremities. On the 29th of November the patient had a severe fright, and her temperature rose to 107.8°. After this the records of temperature are so extraordinary as to suggest fraud, were it not that there was no way in which a fraud could have been effective. At midnight of the same day three successive records, taken at short intervals, read 111°, 108°, 98°. On November 30th the temperature in the right axilla was 108°; the left, at the same time, 99.4°. At midnight the temperature was 98° on the right side and 108° on the left. Similar local maxima, varying from hour to hour, were observed on the 1st, 2d, and 3d of December; after which the records are not given. On November 30th the patient had several spasms simulating tetanus, probably hysterical opisthotonos. On December 1st, together with headache and nausea, there was a peculiar rhythmical movement of the eyelids, alternate elevation and depression. There was left internal strabismus, and sluggish reaction to the light of the right pupil. Throbbing pain at the vertex increased by pressure. On December 3d there were frequent spasms, with muffled heart-sounds; pulse at the wrist imperceptible. On December 4th the patient became delirious, and continued so until the 13th. The plantar and patellar reflexes were both absent; there was cutaneous anæsthesia, incontinence of urine and fæces. After the 13th these symptoms disappeared, and the patient began slowly to improve. But she was not fully recovered until April.

Clemrow considered the hyperpyrexias to have been local, and not extending throughout the body.

In the *Lancet*, for 1879, Donkin related the case of a girl of nineteen, who, during convalescence from a mild typhoid fever, had, at frequent intervals, temperatures of 108° or 110°. These were of short duration, and unaccompanied by other symptoms than a sensation of heat.

In another case, observed by the same writer, from the 20th of May to the 20th of June the temperature every morning and evening varied between 101.8° and 106.8°.

Donkin quotes similar cases from Creig Smith, Cliffe, and Meade. The last, like Donkin's own case, was also a girl convalescent from typhoid, whose temperature for a month kept incessantly varying from 103° to 109°, sometimes in fifteen minutes would run up to 111°. In these English cases the temperature was always taken in the axilla.

In the *Gazette Hebdomadaire*, for 1886, Debove describes a patient who, every day for a month and without other symptom, presented morning and evening a temperature of 39.5° C. This was in November. In December the temperature rose to 40°, on the 17th of January was 41.4°, and on the 25th reached a final maximum of 41.4°. After this it slowly fell, and became normal on the 30th. During this period of three months the morning and evening temperatures were almost always alike: occasionally one or the other was higher by one-tenth or two-tenths of a degree. This prolonged hyperpyrexia resulted in no emaciation or loss of strength.

In 1886 Barié described a case (also in *Gazette Hebdomadaire*), a severely hysterical young woman, servant at Bicêtre. She was subject to frequent convulsive attacks, transient paralyses, profound disorders of sensibility. One morning, after a violent convulsion, she became completely hemiplegic, on the left side, except the face, as regarded both mobility and sensibility. After this she had thirty convulsive attacks in the course of twelve days. Sometimes for two or three days together she would remain in a state of complete mutism, without eating and also without urinating. All remedial measures failed, and the physician contented himself with simple observation. One morning, after a violent convulsive attack, the temperature in the axilla was found to be 39° C. From this time, for twenty days, there was permanent fever, as measured both in the axilla and rectum. Evening temperature was usually higher than morning by some tenths of a degree, but on five days the morning temperature was the highest. There was no functional disturbance, and the tongue remained moist. The fever was highest on the days of the attacks, but persisted on the other days also. On the twentieth day sudden defervescence occurred, the patient remaining otherwise the same, neither better nor worse.

In the "Periscope" of the *Journal of Nervous and Mental*

Disease, for February, 1890, is described a case of hysterical pseudo-phthisis where, during three days, the temperature varied from 103° to 104° F.; on the fourth day it rose to 113°, and the patient became slightly delirious. In an hour the temperature fell to 108°; in the evening was 106.3°. On the next day it again rose to 113°, but fell in an hour to 99.5°. During the next few days the temperature varied from 101.3° to 103.1°, and then became normal. The symptoms had begun with an attack of hæmoptysis, which was followed by severe dyspnœa, cyanosis, and apparently threatened asphyxia several times during the night. During the next two months the same group of symptoms was repeated several times with complete absence of physical signs of phthisis. There was retention of urine.

The most interesting cases quoted in the thesis of Henri Fabre are two, of simulated meningitis, one of apparently severe pulmonary disease. The first of these, a young woman of twenty-four, who had previously suffered from chorea and nervous aphonia, was admitted to the hospital with a temperature of 39.5° C. Her face was swollen and congested, eyes closed on account of an intense photophobia. The head was retracted completely, cephalalgia violent, insomnia and cries, abdomen retracted, constipation absolute, meningitic streak easily developed, severe generalized hyperæsthesia, knee-jerk little modified, no morbid condition discoverable in lungs, heart, or kidneys. During ten days the patient remained in about the same condition: prostrated, eyebrows contracted, pupils contracted but equal, five or six times bilious vomiting without effort (having all the appearance of cerebral vomiting). A diagnosis was made of tubercular meningitis, and (but with little hope of doing any good) leeches were applied behind the ears and calomel administered. On the tenth day the patient was found sleeping naturally, and, on being aroused, ceased to complain of the pain in her head. The temperature had fallen to 38° C. In a few days more the patient was fully convalescent, but on first getting up was affected by a transient paraplegia.

The history of the second case closely resembled the first.

I have myself seen a similar case in the service of Cornil at La Charité, and, curiously enough, the same patient returned, a year later, with the same group of symptoms, and

her personality being recognized, the diagnosis was the second time at once correctly made.

The case of febrile hysteric dyspnœa related by Fabre is as follows: The patient was a woman of twenty-six; admitted to the hospital with an evening temperature of 39° C. and a dyspnœa of five or six days' duration. There were thirty-five to forty respirations a minute, but unaccompanied by trace of cyanosis. The most careful auscultation failed to discover any lesion of either lungs or heart, and the absence of albuminuria was held to exclude a uræmic origin to the dyspnœa. The fever continued for twenty days, being extremely irregular, with occasional intermissions of normal temperature, followed by a rise to 39° or 40° or over. On the twentieth day occurred an abrupt defervescence, and at the same time the dyspnœa ceased.

The recognition of hysterical fever as a distinct clinical affection has been much facilitated by recent researches on the relations of the cerebro-spinal nerve-centres to the temperatures (general or local) of the body. As every one knows, these researches were initiated by the famous observation of Sir Benjamin Brodie, on a rise of temperature in a few hours to 111° F., in a patient who had sustained a fracture of the spine, with traumatic section of the cord. This observation was published in the *Medico-Chirurgical Transactions* in 1837.

The researches of Tscheschin, in 1866, are equally famous and well known. In some respects they seem in contradiction with Brodie's clinical observation: for when, in animals, this experimenter cut the spinal cord below the medulla, the temperature of the body fell; but if the section were made between the medulla and the pons, the temperature rose excessively.

The more exact experiments of Horatio Wood, in his beautiful researches on fever, published in 1880, demonstrated that when the spinal cord was cut anywhere between the level of the third and second cervical vertebra there was at first an enormous increase of heat-dissipation, correlative with the general vaso-motor paralysis; that in forty-eight hours this was followed by a diminution in the dissipation of heat, but also a diminution in heat-production, so that, as had been before observed, the net result was a fall of body temperature. Wood also observed the rise of temperature consecutive to section of the cord between the medulla and pons. He accepts the in-

ference drawn from the facts by Tscheschin, that there exists in the medulla some nerve centre or centres whose influence tends to stimulate the production of heat in the thermo-genetic tissues, namely, the muscles; that this influence is habitually restrained by that moderating centres in the pons or above it, and that the rise of temperature observed in the last experiment is due to the withdrawal of this moderating influence from the real heat centres. More recent experiments have extended the field of experiment and inquiry. Eulenburg and Landois showed that excitation of one cerebral hemisphere is followed by a local rise of temperature of the limbs of the opposite side. These experimenters made no observations on the general temperature. In 1884, Charles Richet (Compt. Rend. Societé Biol., 22 Mars, 1884) pricked one cerebral hemisphere of a rabbit with a steel pen which perforated the cranium, and found in the course of two hours that the rectal temperature rose from 39.5° C. to 40.4°. The next day, when the temperature had fallen to 39.2°, a nerve pricking caused a rise to 42.8°. The animal died in the night, presumably of the hyperpyrexia, as no brain lesions were discovered to explain the death. It was found that the pin had penetrated to a spot situated three or four millimetres in front of the corpus striatum.

A little later, Schreiber[1] found that a rise of temperature occurred after lesion of any part of the pons, of the cerebral peduncles, cerebrum or cerebellum, provided the animal operated on were protected from the radiation of heat by wrapping in cotton wool. In 1885, Aronsohn and Sachs in Germany, and Dr. Isaac Ott in America, began almost simultaneously, but quite independently of each other, to search for heat-regulating centres in the brain. The German observers[2] trepanned rabbits at the juncture of the sagittal and coronal sutures, and entered the brain with a needle, three millimetres broad, at a point about one millimetre outside the longitudinal sinus. A carbolized dressing was immediately applied, and the well-being of the animals seemed to remain undisturbed.

When the operation was performed on the cerebrum anterior to the Rolandic convolutions, no effect on the temperature was observed. But the punctures which passed to the base of the brain, from the point of junction of the coronal and sagittal

[1] *Pfluger's Archiv.*, viii., S. 576.

[2] *Pfluger's Archiv.*, 1885.

sutures were always followed by an enormous rise of temperature. If the puncture only penetrated the cortex cerebri, no effect on temperature was produced. Electrical irritation of the susceptible region, *i.e.*, the tissue just in front or on the outer side of the corpus striatum, also caused a rise of temperature. An increased excretion of nitrogen was observed during this artificial fever, so an increased heat-production was inferred, but no calometrical observations were made.

These difficult observations were, however, made by Ott,[1] and add greatly to the value of his experiments on the brain.

Ott established four localities at the base of the brain whose puncture, and consequent irritation, was followed by a rise of body temperature. These were, at a point just within the anterior part of the corpus striatum; a second point between the corpus striatum and the thalamus; a third at the anterior part of the thalamus; and a fourth at the point of decussation of motor fibres at the nib of the calamus in the medulla. In the fever consecutive to irritation of these centres, there is at first an increase of both heat-production and heat-dissipation but both soon fall below normal, though fever continues. In addition to these centres, however, Ott discovered two others on the cortex; one at the point of juncture of the supra sylvian and post sylvian fissure; the other in the neighborhood of the cruciate sulcus, *i.e.*, over the Rolandic convolutions.

When either of these cortical centres were irritated, temperature was depressed. If, on the other hand, they were removed by slicing and subsequent washing with carbolized water, the temperature rose.

From the total result of his experiments, Ott infers that the basal centres, like those of the spinal cord, habitually stimulate the production of heat; are thermogenetic centres. But those of the cortex, the sylvian and cruciate, habitually restrain the activity of these lower centres, and may therefore be called thermotaxic.

Under certain circumstances the striate and extra striate centres may also be thermotaxic, and moderate the spinal centres below them. They have, therefore, a mixed character or function.

Girard[2] confirmed the results of Ott's experiments on the

[1] *Journal Mental Disease*, 1888. [2] *Archives of Physiol.*, 1886 and 1888.

corpus striatum, and also observed a rise of temperature to follow punctures at various localities in the posterior part of the brain, but none when these were made anteriorly. The fever was attended by increased elimination of nitrogen in the urine, and was controlled by antipyrine. Rise of temperature was also induced by faradising the striated bodies for half an hour with needles insulated to their tips.

Horatio Wood, also, in thirteen out of fourteen experiments, found that localized destructions of tissue just behind the crucial sulcus, thus compromising Hitzig's region, were followed by a rise of temperature and decided increase of heat-production.

A curious confirmation of the foregoing observations is offered by Zawadowski,[1] who found that antipyrine ceases to reduce temperature if administered after section of the spinal cord at the atlas, an operation which removes the inhibitory influence of the brain from the thermogenetic centres of the cord.

The interest of the foregoing observations is very great in their bearing on the general theory of fever. In accordance with them, all fever can finally be ascribed to derangement of the central nervous apparatus, which controls the generation of heat in the muscles, the latter being the ultimate thermogenetic apparatus. Hence, the striking fact, that the cerebral centres so far established as regulating the production of heat, are chiefly situated on the motor tracts, namely the Rolandic convolutions, the striate centres, and the medulla.

In zymotic fever the thermogenetic centres would be irritated by the poison circulating in the blood; in traumatic, perhaps also in inflammatory fever, the same result is produced by irritation of peripheric nerves; in hysteria there would be paralysis of the cortical thermotaxic inhibitory centres rather than excitation of the basal thermogenetic centres.[2] Reflex fevers, like urethral and worm fever, might be supposed to imply on the other hand direct irritation of the thermogenetic centres.

This paralysis would then enter into the entire series of hysterical phenomena, which depend upon loss of cortical control over lower centres. It becomes analogous to the loss of cortical control over subcortical vaso-motor centres, upon

[1] *Centralblatt f. medicim-wissen*, 1888.

[2] W. Hall White, loc. cit.

which Meynert has so strongly insisted, and nevertheless it is not to be resolved into a vaso-motor phenomenon. For it has been shown, especially in some experiments of Wood's, that the vaso-motor medullary centres are not affected in these artificial fevers, and respond as usual to an irritation of the sciatic nerve.

A danger attends the recognition of any group of clinical symptoms as hysterical. It is the danger of ascribing to hysteria, symptoms which are really caused by organic disease. This is even more serious than the opposite error of interpreting as the result of organic disease, symptoms really due to hysteria. The diagnosis is, therefore, always important, and often delicate and difficult. It would be impracticable in this place to analyze the elements of diagnosis in regard to each case which might be simulated. But this may always be remembered: Exclusion of the grave organic lesion which may be simulated, does not necessarily exclude the origin of the disorder in some lesser lesion, which may even entirely disappear, while the storm which has been aroused continues. The type of such a sequence is offered by the prolonged hysterical neuralgias which may originate in a slight sprain (traumatic hysteria).

In the case which forms the basis of this paper, I think it is not at all improbable that the last series of accidents originated in a slight hemorrhage into the cortex of the ovary, occurring at the time of the arrested menstruation. A permanent ovarian irritation or irritability existed, manifested by the persistent recurrence of menorrhagias, in the absence of all uterine disease. It seems as if this would be sufficient to explain the entire series of phenomena, itself being an expression of a grave hysterical diathesis.

MODERN FEMALE INVALIDISM.[1]

In the course of the preparation of the Shattuck Lecture for 1895, on "The New England Invalid," recently published in the Journal, its author, Dr. R. T. Edes, obtained a letter from Dr. Mary Putnam Jacobi, which by his and her permission we print. The letter was not originally prepared for publication. Criticisms might perhaps be made upon it as to its details, but as a whole it is very interesting and suggestive, and we commend it to our readers.

Hotel San Marko, St. Augustine, Fla.

Dr. Robert T. Edes. *Dear Sir:*—Dr. Morton handed me your letter to her some two or three weeks ago, asking me to comment on it. I do not know that my comments will be of any use; still, as Dr. Morton asked me to do so, I will offer a few suggestions.

In the first place, it seems to me that this entire question needs to be dealt with on a much larger scale, and from a more anthropologic standpoint than is usually the case. Impairment of reproductive function through disease, or imperfect development of the reproductive organs, is a race fact of the greatest importance; and much evidently depends on quite a combination of conditions. To assume, as good old Miss Beecher did, that all the troubles connected with reproductive organs can be explained by the habit of wearing many petticoats, is to rely upon a most superficial and inadequate explanation. Miss Beecher suggested, as a remedy for the evil, a hoop shirt, which actually and by independent agency came into fashion a few years later; but I doubt, if it greatly changed the conditions Miss Beecher was considering.

In the most general sense, and apart from specific infections and mechanical injury, utero-ovarian disease is evidently traceable to imperfect development; and it cannot be denied that this is alarmingly prevalent among American, and especially among New England girls. But I think it is putting the cart before the horse to assert that this imperfect development of the reproductive organs and corresponding nerve centres, is due to over-stimulus, over-education of the intellectual centres.

The first question to ask is, Why are the latter centres in such a state of

[1] Reprinted from the *Boston Medical and Surgical Journal*, 1895.

activity, that they spontaneously demand opportunities for greater exercise, or eagerly respond to the systematic education, which for the first time in the history of the world, is offered to masses of girls?

My own answer would be complex. In the first place, it seems to me that reproductive failure, or rather the relative degree of it which is observed, and which is shown is amenorrhea, dysmenorrhea, the ovaro-metritis of menstrual subinvolution, in sterility after the birth of one child, that these phenomena are fundamentally phenomena of acclimation. On that very account they should be more conspicuous in New England, whose rigorous climate differs from that of Old England than does the climate of the Middle States. In the South and West the influence of malaria replaces the influence of a colder climate.

I am immediately reminded that the climate of Canada is more rigorous than that of New England; yet both French and English Canadians are remarkable for fertility, and I think are much less subject to uterine disease.

It is evident, therefore, that other elements come in to complicate one that always exists to some extent unless counteracted, when a race is transplanted to a foreign climate, as the English in India. The second condition I would refer to the special circumstances of the settlement of New England. For a hundred years there were conditions of peculiar hardships; and the present inhabitants are in many cases the descendents of those who during that first century struggled at once with cold, poor food, confinement to mean houses for two-thirds of the year, epidemic disease (small-pox) the normally depressive influence of constant apprehension from the Indians, and constant gloom from a religion as ferocious as the character of the Indian enemy.

In Canada during this same century, there was relatively little settlement by families who left descendants; it was colonization by monks and nuns and soldiers; and the half-breed illicit progeny of the latter had one ancestral foot well planted in the soil. The real colonization of Canada began in the next century, when conditions of life were beginning to be easier. On the other hand—and here is a curiously cross influence, which I think we must take into account—increasing care of life means diminished infant mortality and a constant survival of individuals with a debility which, though sheltered during childhood, manifests itself irresistibly during adolescence. The powers of life, sustained for the more or less adequate fulfilment of individual functions, flag in reproduction nutrition; the wave ebbs from its outermost expansion. Another cross influence comes in here, which is almost paradoxical: namely, that people of exceptional longevity are now known not to transmit as much vigor to offspring as do those of only average strength. Hence, only those who survived exceptionally severe conditions, such as surrounded the early New England colonists, may be presumed, like the people who to-day live to eighty and over, to have absorbed or consumed the nutritive forces of the organism in the interest of individual functions; leaving less for the "supplementary waves" of reproductive nutrition, and less for the offspring developed in these.

How often does every physician receive in his consulting office frail girls brought by robust mothers, who cannot understand this fragility when they themselves have never been ill in their lives? This is the converse of

what is also seen in the robust sons of sick mothers who seem to have absorbed the strength of the parent stem. The influence of tubercular inheritance is especially important both in the development of uterine disease, of the anemia which often precedes this, and of the neuroses which may either precede, originate or replace this. I think cervical catarrh in young girls, with passive pelvic congestions and uterine displacements from relaxation of ligaments and of the pelvic floor, may almost invariably be traced to a tubercular family diathesis as it frequently precedes tubercular disease in the individual.

Tubercular disease makes a *premier étape* of degeneration, that which gives a susceptibility to an external agent; hence is less profound than the spontaneous degenerations of the grave neuroses. Yet how many of the latter also, at their inception, merely imply a susceptibility to physiological or other irritants and would be averted were the nature of these better understood!

I believe that ovarian disease on the other hand, though always originating in some degree of fundal endometritis, whenever it assumes marked prominence or independence, is always rooted in a neurotic diathesis rather than in a tubercular. Many of the nervous or even mental disorders often assigned to "reflex ovarian irritation" are the direct expression of cerebral disease or cerebral malnutrition, which also causes the vaso-motor paresis in the vascular territory of the ovaries; at least, this is my explanation.

Here, again, the individuals who would in former generations have died of tubercular disease in infancy or early adolescence are preserved by a better hygiene, therapeutics or climatic adjustments, to mature life, but on penalty of suffering from uterine disease excited either by celibacy or childbirth.

And, again, it was not necessary to wait for Grasset to show (as in his essay on "Tuberculosis and Hysteria"[1])that the neuropathic constitution in itself is often a transformed tubercular diathesis.

The wide prevalence of consumption in New England, due to climate, to poor food, and to imperfect acclimation among ancestors, is certainly a very important source of uterine disease, and attendant or equivalent neuroses in the present generation.

I believe it is also true that the imperfect development of reproductive organs, nerve centres, and correlatively of sexual instincts, is one reason that the intellectual life of women, and the cerebral cortex, has in the present generation become more active. To suppose that cerebral activity could dwarf sexual activity (which is often alleged) is absurd, or rather, though theoretically conceivable, is in contradiction with known facts. The one fact, now noted by ethnologists, that sexual passion is far more highly developed among highly civilized peoples than among savages, shows that normally the two poles of existence develop *pari passu* and not in antagonism to each other. Detailed proof could easily be furnished were it necessary.

But until now, women have not held a normal position as complete human beings; their mental activity, though often considerable has been spontaneous, untrained, unsubjected to systematic educational drill. I think the flagging of reproductive activities, due to temporary impairment of race vitality, has facilitated this extraordinary new departure in the *régime* of the race, whereby

[1] Brain, 1880.

the sex whose brain has been hitherto neglected, is to-day educated, stimulated, often unfortunately forced.

But because this new departure is a race innovation, it undoubtedly involves difficulties and dangers, risks a certain dislocation of organic adjustment, which can only be gradually triumphed over. It requires the most careful study of individual cases, and recollection of three facts. First, that the girls already in possession of the most active, responsive and readily educatable brains, may be essentially deficient in general organic force, and especially as manifested in the reproductive organs, hence unprovided with the undercurrent of sexual strength which is needed to healthfully support cerebral activities. Second, that the other girls—and there are to-day not a few of this class—who are not only mentally active but seem physically sound and strong, may not have the final reproductive strength; their menstrual life is healthy, but they may either break down in child-bearing or have delicate children. Third, that the reproductive imperfection in question may show itself at first by no more tangible symptoms than moral incapacity for love or marriage, or fantastic perversity of sentiment in regard to these fundamental interests, this incapacity frequently involving or determining social situations that react most disastrously upon the health of the "highly strung" individual.

With all these race and constitutional complications, when educational systems are adopted which not only ignore such general considerations but violate the most elementary principles of ordinary hygiene; when brains which are not only immature but female, and whose stock of inherited capacity for trained work has all been derived from the parent of the opposite sex; when such brains are submitted to an often illogical cramming; when food is inadequate and exercise absolutely neglected; when hours of work are imposed which no adult woman would bear, and few men; when all this work is carried on under the stimulus of high-pressure competition, emulation, vanity, sometimes fear; when hundreds of girls are shut up together in the exciting atmosphere of a college life, so that their nerves are mutually reacting on each other, —under all these circumstances it is not at all wonderful that towards the close of adolescence so many girls exhibit constitutional debility and uterine disease.

It must, however, be noted, and contrary to what might theoretically be expected, that the influence of superior education, although occasionally seeming to be detrimental, is far less so than any other observed agency. Where there is to be trouble, this is always distinctly foreshadowed at or before the ages of sixteen, seventeen, eighteen, when the college education begins. My own statistics, as collected in the essay on "Rest in Menstruation," and also those which have since been collected by college alumnæ, all show that the least ill-health is found among the women who have been most highly educated. Of course, the fact partly shows that only healthy girls complete their course and graduate; others fall away earlier. At all events, the college-bred women are still so much in the minority that the general statistics are hardly affected by them; yet physicians often write as if these constituted the mass of nervous invalids.

But the list of causes of the special invalidism of the century is not yet ended. Every city physician who has also seen country people must have noticed that while mechanical injuries from childbirth are rather more com-

mon among country people, their influence is apt to remain limited to mechanical discomfort. But a much less degree of injury in city women excites, or is liable to excite, a protean swarm of nervous disorders. The French comment emphatically on this contrast between the Parisian and peasant women. Evidently this implies more delicately strung nervous organization, in more unstable equilibrium, as more developed among the inhabitants of cities, or specifically among the women who have been freed from manual labor. I think Tolstoi is quite right in asserting that such freedom is a curse to the "upper classes."

If the excessive drudgery of New England ancestors under unfavorable conditions weakened the constitution of their descendants, the excessive luxury of these descendants is certainly a second cause of weakness. I am not speaking now of coarse and unreasonable luxury, but of the refined and delicate ease of life and sensibility in which so many thousands now contrive to live—up to a certain point an advantage and a grace, beyond this a dangerous effeminacy. In manual drudgery, or in Puritan asceticism, there are dangers from exhaustion, depression, or gloom; but there is at least a discipline, an enforced stoicism, which is of immense value in bearing toil, trouble or shock. To-day stoicism has vanished from education, as asceticism from creeds; it is considered natural and almost laudable to break down under all conceivable varieties of strain—a winter dissipation, a houseful of servants, a quarrel with a female friend, not to speak of more legitimate reasons.

Women who expect to go to bed at every menstrual period expect to collapse if by chance they find themselves on their feet for a few hours during such a crisis. Constantly considering their nerves, urged to consider them by well-intentioned but short-sighted advisers, they pretty soon become nothing but bundles of nerves. They suffer from lack of the wholesome neglect to which their grandmothers were habitually consigned; too much attention is paid to women as objects, while yet they remain in too many cases insufficiently prepared to act as independent subjects. A healthy objectivity is one of the greatest desiderata for modern women. To knock the nonsense out of them, to direct attention from self, to substitute a cosmic horizon for that of their own feelings, who does not know the importance of this for thousands of hysterical women? and equally the impossibility of attaining it?

I think, finally, it is in the increased attention paid to women, and especially in their new function as lucrative patients, scarcely imagined a hundred years ago, that we find explanation of much ill-health among women, freshly discovered to-day, but which always excited, and which is often due to conditions arising among men, and not therefore new. Shattered nervous systems are inherited by girls from the alcoholism of the fathers; gonorrhea contracted by wives from husbands; sterility due to licentiousness in which the innocent woman may have no share; enforced celibacy due to bad social arrangements; occasionally, though less and less frequently, childbirths too close together; certainly all these causes of ill-health to women have existed for centuries. I think the peculiarity of the present time is that now attention is being drawn to the special effects produced upon women by these general causes. All of which is respectfully submitted.

Very truly yours,

MARY PUTNAM JACOBI.

A SUGGESTION IN REGARD TO SUGGESTIVE THERAPEUTICS.[1]

NOTWITHSTANDING the number of elaborate and ingenious theories which we now possess in regard to the phenomena of hypnotism, every medical discussion on the subject seems to reveal the absence of sufficiently definite formulæ for practical medical guidance. Neurologists will often state that they have hypnotized every patient in their wards for a while in order to see what would come of it; and when nothing came of it, they abandoned the method. This way of experimenting resembles that practised some thirty years ago in the Paris hospitals by Briquet. To ascertain the therapeutic value of large doses of quinine, he gave thirty or forty grains to every patient in his service. Noticing, after this medication, that such patients as were at the time lucky enough to be suffering from acute inflammatory rheumatism were improved, he assumed that large doses of quinine were a specific for acute rheumatism. And so they continued to be until later they were displaced by the salicylates. Without discussing in detail the numerous reports of already recorded cases, and without submitting to a fresh analysis, the already so often scrutinized rival doctrines of Charcot, Bernheim, Heidenhain, Myers, and others, we may, I think, from consideration of generally accepted facts, formulate this precise statement:

Hypnotic suggestion acts upon specific cases of either pain or disability which depend upon morbidly persistent organic memories of pain or disability. It is now generally conceded that memory is a general property of organic tissue, in virtue of which any event or series of events which has once occurred among its elements tends to reproduce itself indefinitely until interrupted

[1] Reprinted from *The New York Medical Journal*, 1898.

by some new influence. From this point of view, organic memory constitutes one mode of the still more general property of matter which we call inertia.

In organisms possessed of nerve centres this general rudimentary tissue memory becomes complicated by a special circumstance. This is, that the events which transpire in the general somatic tissues excite secondary events in the nerve centres, because, by means of the nerve filaments connecting the periphery with the centre, an impression made upon the former is transmitted to the latter, is in a manner perceived by the latter. This secondary impression constitutes a record of the organic event, and when it is made upon the brain cortex of human beings or even of the higher mammals, becomes what is especially known as memory.

Where, as in human beings, there are two systems of nerves, the cerebro-spinal of voluntary life and the sympathetic system for the vegetative life, the recorded memories of the former are so predominant and important that those of the latter fall entirely in the background.

The evidence of the record is extremely indistinct, because it does not rise into active consciousness. No neuromuscular action passes unperceived unless through mental preoccupation with other things. But all normal visceral processes do so habitually. Moreover, in numerous cases degenerative or malignant disease may, unperceived, effect extensive structural ravages in organs hidden from view, because the disease fails to excite the pain which is alleged to be Nature's warning signal of danger. If it be so, her sentinel often sleeps on his post, and even more often cries "Wolf!" with all the mendacity of the boy in the fable. The so-called conservative function of pain may have been present as such in the original intention of Nature; but in actual fact, this intention is woefully overlaid and distorted, as in so much else of the good dame's blunderings.[1]

If, as I believe to be the case, hypnotic suggestion acts specifically upon the memories of events recorded in or by the brain cortex, the relative obscurity of the record for visceral and nutritive processes would be a reason to greatly limit the efficacy of

[1] Facts like these recall the bold speculation of C. S. Peirce, that the realm of Law is not coextensive with the universe, but only extends over a certain, however large, area, beyond which is Chance.

the method in regard to these. Experience shows, indeed, that hypnotism is not often efficacious in trophic disorders. On the other hand, there are a few cases, apparently well authenticated, where visible trophic lesions, like ulcers, have yielded to hypnotic suggestion. This is sufficient to show that the nutritive events which have led to the lesion have been recorded in the brain, and may therefore possibly be reversed by an influence exercised directly upon the brain and subsequently emanating from it.

The most interesting field for hypnotic therapeutics is admittedly that offered by the sphere of animal life. The most definite application is to cases of pain or of some specific functional disability, which have originated in an individual morbid occasion whose efforts by good rights should have disappeared with the cessation of their cause, but which have, on the contrary, persisted.

A typical case is the persistent pain of hysterical joints, initiated by a sprain, but capable of lasting months after all symptoms of the sprain have subsided. I do not think we are much helped by calling such pains imaginary, or by saying that they "illustrate the influence of the mind over the body."

But let us look at it in another way, and instead of talking about the mind, which perceives or feels the pain, consider the brain, which records the event that has transpired within the cerebro-spinal sphere. In any case, and for the person with the most healthy nervous system the brain record will be made, for the accident can be remembered. But in the healthy person the sensory impression rapidly fades out of consciousness, out of memory, and only the accessory circumstances are retained. The injured person remembers the fact that he had sprained his foot, but he ceases to remember how he felt when he did it, and can not by any effort reproduce the conscious sensation. To use a customary terminology, the sensation sinks out of his supraliminal consciousness and falls back into the subliminal consciousness; it ceases to be a part of his present existence, and becomes only an item of his past experience. But as his total personality is made up of his present and past experience taken together, it may be said that no sensation once experienced is ever entirely lost. Normally, the past experience is submerged by the present, which is, perhaps, equivalent to saying that when fresh intracortical processes are being sustained in full vital activity, there is

no room left for the monotonous repetition of an isolated process which has been excited by a peripheric stimulus. And of all the processes which may be so excited, those resulting in conscious sensation are evidently the most easily submerged, for a past sensation is immeasurably more difficult to reproduce than a past emotion or a past idea.

With a normal brain it is, in fact, absolutely impossible to reproduce a sensation without repeating the stimulus which occasioned it.

Abnormally, however, this reproduction or this persistence occurs under several circumstances.

If the total vitality of the brain be for the moment diminished, the intracerebral process excited by a peripheric irritation assumes a disproportionate intensity to other intracerebral processes. Hence the familiar observation of the generation of persistent pain, neuralgiform or other, in anæmic, exhausted, or malnourished persons. Or if, from the original constitution of the brain and character, sensations habitually occupy a disproportionately important position in consciousness, the occurrence of a painful sensation tends to persist because the phases of conscious life which arise in incessant succession, so far from drowning out and submerging the sensation, are themselves invaded and absorbed by it. The emotions and ideas become enlisted on the side of the sensation, amplifying it far beyond its original scope, and by just as much tending toward its indefinite perpetuation. This seems to be the sequence of things in constitutional hysterics, to whose consciousness bodily sensations are always disproportionately important; or further, the circumstances accessory to the production of the sensation may at the outset enlist the other cerebral activities to the amplification of the sensation. Thus, when there has been such just cause for fright or shock, as often happens in traumatisms, or even when the pain has originated in an unfamiliar and therefore possibly mysterious procedure, such as the plaster-of-Paris apparatus for fracture, which caused a severe hysterical neurosis in a case related by Charcot. I have myself seen a similar case.

In cases like the foregoing, the present conscious life of the brain is permanently dominated by a past experience, morbid, not in its occurrence, but in its persistence. Admittedly an immense number of pains and painful affections are to be so

explained. And it is precisely such pains which constitute the best objective for the therapeutic influence of hypnotism.

When this is successful it acts in two ways: By the sleep wherein is initiated the amnesia characteristic of normal sleep; and by the suggestion, which focuses upon a narrow point, and therefore with great intensity, all the cerebral activity of the present moment. Without the suggestion, the hypnotic amnesia would be as transient as is the amnesia of normal sleep. But without this induced amnesia, which increases the suggestibility of the patient in a manner confessedly most mysterious, the suggestion would not obtain a sufficient foothold in the mind—*i.e.*, would not be recorded with sufficient depth in the brain.

The occasional successes of the faith curists, however, who operate by suggestion unaided by the sleep, show that the former alone is indispensable. The same proof is offered by more ordinary forms of suggestion. Evidently the imposed suggestion acts along the same lines as do the normal cerebral activities in the cases where a sensation promptly ceases with the cessation of its cause. The mind is made to think of something else. It is a familiar fact that under sufficiently intense mental preoccupation, severe irritation may fail to awaken sensations even at the moment of their maximum operation: thus wounds may be unfelt during the excitement of battle. Into the intense activity of the sum total of the brain processes the isolated process at the basis of the sensation is unable to introdue. If this paradoxical inability be possible for a present sensation, or, more correctly, for a sensory process whose cause is at the moment operative, it is more easily conceivable for the central brain process which is only reviving a past sensation. The reason that we ever forget anything is because, so long as the brain is alive, it is compelled at every moment to be occupied with something new. The necessity may be at least metaphorically compared to that by which mediæval writers were compelled to efface the writings already inscribed on parchments in order to utilize these for new inscriptions.

It does not seem as if chronic disease, certainly not chronic pain, had ever been included within the original scheme of Nature. Sudden injuries from external agents are evidently anticipated. These are to be as promptly arrested, resisted, and rejected. The wound may be fatal because overwhelming; but

if less than fatal, it is expected to heal, and be forgotten as completely as a landslide on a mountain which has become overgrown with grass and trees. Modern life has become enormously enlarged by the modern strengthening and amplification of memory. Perhaps the vast enjoyments to the race of the historical memory of past ages, rescued by it from oblivion, are purchased in part at the expense of an inconvenient increase in the tenacity of the personal memory, and especially of personal organic memories, where forgetfulness is so often more desirable. The persistence of a disturbing past sensation tends to inhibit other cerebral activities, exactly as these, when fully developed, tend to submerge it. Hence, the well-known tendency to the constant increase in the range of morbid phenomena under the depressing influence of a single pain, either mental or physical.

It is cases of neurasthenia which have developed in consequence of such insistent insults to the brain that should be justifiable to hypnotism; while neurasthenia due to malnutrition, and with no primary cortical disturbance, rests on an entirely different basis, and demands different handling.

An extremely interesting circumstance about the post-hypnotic suggestion is, that it seems to encounter a definite, almost a measurable, amount of resistance, and this can only gradually be overcome. During the hypnotic sleep it is often possible to impose the wildest ideas upon the patient's acceptance those most contradictory to all his past experience. But the post-hypnotic suggestion directed against his past and deeply rooted experience of pain can not proceed so abruptly. In many cases (I do not say in all) it appears that if the patient be assured that on awakening he will be entirely relieved of his pain, the suggestion will fail. If, on the other hand, he be told that his pain will be diminished, this holds true; and in successive *séances*, by successive diminutions, the vanishing point will be reached, or, to judge from some of the recorded cases, the assurance of complete relief will be followed by partial relief, and the apparent failure of the prophecy does not seem to occasion enough skepticism to prevent successive suggestions from accomplishing the purpose. These facts of resistance, gradually overcome, and which seem sufficiently well attested, point to the physical basis of the entire mental experience. In some mysterious way, whether dynamically or otherwise, it is unnecessary to speculate,

a material record has been effected in the tissue of the brain, and this can only be changed through newly induced intracerebral processes. The facts may be utilized to throw light on the processes of conviction in regard to other mental phenomena than sensations. No belief once rooted in the mind is ever changed suddenly and *in toto*. If a man is being converted from the belief in eternal punishment, he first surrenders for ordination, then infant damnation, and finally, little by little, the whole blessed doctrine is upheaved, enucleated, and falls away under the pressure of mental activities occupied with something else. These grow beneath a fixed idea, intrinsically repellent, like granulation tissue beneath a foreign body in a wound, and push it, little by little to the surface.

So with the morbid idea which constitutes pain, when, indeed pain is a morbid idea. When it is not—*i. e.*, when it is the normal response to a peripheric irritation—hypnotism can only act by securing intense mental preoccupation, and the hypnotic sleep must be prolonged during the occasion. In this way it seems sometimes to happen that the hypnotizer is able to overcome the violent suffering of childbirth.

The return of pain on awakening from an hypnotic sleep implies that the sensation which could not intrude itself upon consciousness during the intense preoccupation of that sleep is able to do so when this preoccupation relaxes, and the ordinary plane of consciousness is resumed. When a verbal suggestion made during the sleep begins to operate afterward, it is clear that the idea embodied in the suggestion has been apperceived, and has begun to modify the sensation. If the physical basis or aspect of the idea be, as we may provisionally conclude, some particular "set" of communications between cortical neurones, it should be expected that this must change with the apperception of every new idea, or, to put it in another way, the existence in consciousness of a new idea implies that the "set" of the combinations which coincided with the previous idea must have changed. In trying to persuade a person by ordinary suggestion to cease feeling a pain, because it is irrational to feel a pain for which there is at the moment no cause, the difficulty lies in the fact that to the "set" of combinations which begins to be established by the apperception of this suggestion is opposed the "set" which underlies the conscious sensation, so that the former does

not succeed in establishing itself widely enough in the brain. It must, however, establish itself to a certain extent, or it could not be apperceived, recognized, at all. The same suggestion repeated during the amnesia of the hypnotic sleep encounters no opposition, and is therefore enabled to obtain a certain degree of foothold. With the disappearance of the amnesia the brain falls back into its previous "set," and the antihypnotic sensation reappears. But when, as happens in the successful cases, the sensation is weakened, this can only mean that other portions of brain activities have been so aroused as to oppose the extension of the cortical processes which underlie the sensation. The sensation had hitherto existed unchecked, because believed without question. This means that it had seemed perfectly congruous with all the mental phenomena existing at any present moment. And this again, on the postulate that to each mental phenomenon if to any, corresponds some physical process in the brain tissue, must mean that the physical process in the ideo-sensory centre of the cortex did not interfere with the cortical processes otherwise or elsewhere going on, however much it had interfered with, had inhibited, processes which should go on, but which had ceased and had been forgotten from consciousness. The implanted suggestion that at a given present moment the pain did not exist, however really it might have existed in the past, shakes the hitherto unquestioning belief in the externalization of the sensory processes in the cortex; it tends to break up an hallucination to whose support the total brain activities had hitherto unwittingly conspired, and when the physical process to which the suggestion corresponds, the physical "set" of the brain tissue has been repeated often enough—the obstinate "set" engendered by a past and uncontradicted experience gives away. "Because I suffered once is no reason that I suffer now; as no new reason has arisen, it must be that I do not suffer. In fact, I do not suffer." These seem to be the successive phases of consciousness traversed by the subject of a successful post-hypnotic suggestion.

The so-called "subliminal consciousness" is the totality of the past experience, including much ancestral experience. Since, under various influences, elements of this can be revived from the static records in the brain and called up into consciousness—*i. e.*, called from the past into the present—this may be named the

potential self as distinguished from the actual self. In combating an isolated mental phenomenon, as a centrally originated pain, the suggestion appeals to this mass of intricately recorded experience. It must be congruous with this, for if incongruous, it will be influential only so long as the hypnotic sleep lasts, and can have no after-effect. All the previous "sets" of the brain during which no pain has been felt may be revived to establish the predominant habit by whose predominance the isolated experience of pain is gradually crowded out.

If the totality of organic experiences did not so preponderate, the post-hypnotic suggestion would be ineffectual, because it would have no support, no "purchase" in fact. Indeed, it is ineffectual where the elements of the brain cortex are organically diseased, as in insanity, imbecility, or idiocy; where their functional combinations or "sets" are presumably feeble, as in severe hysteria, and in neurasthenia with certain grades of mental feebleness; in the grand neuroses, where ideo-motor or ideo-sensory centres are not primarily involved, as in epilepsy and hysteria major.

It is also ineffectual where the logic of the situation is strongly opposed to the anodyne suggestion, and affirms not only that pain is felt, but that it *ought* to be felt; as in most cases of operative procedure. Post-hypnotic suggestion seems to have no effect upon labor pain, yet it has proved possible in a certain number of cases to maintain an hypnotic sleep, with attendant analgesia throughout the labor. Here, however, it is necessary to maintain a constant reiteration of suggestion after pain; the class of cases characteristically suited to hypnotic therapeutics are those of functional or habit disability. These embrace phobias in the performance of certain voluntary and coordinate and complex actions of tremors and other muscular disabilities, the habitual incompetence of certain unstriped muscular organs—as the vesical sphincter in incontinence of urine, and the muscular coat of the intestine in habitual constipation, the recurrent irritability of other visceral muscles, as of the stomach in incoercible vomiting. The underlying principle in all these cases is evidently the same. The failure to perform a given act having been recorded in the brain, is remembered, and being remembered by an organism whose present vitality is insufficient, tends to repeat itself, the tendency increasing with

every repetition. The tendency lies in the brain, not in the somatic tissues themselves, and the aim of the hypnotic suggestion must be to dislodge the deeply recorded memory of failure and to oppose to it a mental conception of success. The suggestion at first can present this conception as an idea; but, frequently enough repeated, this idea, like all others sufficiently impressive, becomes the personal experience of the person receiving it. As a present experience it is naturally more powerful than the memory of past failure. But as that memory has attained an unnatural predominance, it is necessary to temporarily obliterate it by the device of the hypnotic sleep during the time that the suggestion is being made to the mind, and is also being recorded in the brain. As in regard to the effect of post-hypnotic suggestions upon pain, the definite degree of resistance which is discovered to the suggestion as soon as the amnesia disappears (and this, although at the time the suggestion was made and apperceived neither consciousness nor evidence of resistance was present) proves that the suggestion acts through the indirection of a definite cerebral process, which ultimately counteracts another cerebral process already established. Functional disabilities, however, indicate clearly what is only hinted at in the case of morbid pain—namely, that cerebral activities are involved much in excess of those ascribable to any one sensori-motor centre. Emotions of fear, shame, despair, judgments of impossibility and inconceivability, are evidently all present—in other words, a large area of conscious life is invaded, or even, during the moments that the disability is made manifest, the entire area of consciousness. Hence the peculiar anguish which often attends these states. Yet this may be all resolvable into the single fact that the first experience of functional failure was not forgotten, but morbidly remembered, and that no contrary experience has yet been registered in the brain or consciousness. There seem to be more ways of getting round this form of morbid memory by simple, non-hypnotic suggestion than in the case of pain, and for the reason that the pain is constant, the disability intermittent and variable in degree, so that within the sphere of the voluntary muscles it is possible to build up experience of success by means of finely graduated effort, starting from any given minimum. This can not so readily be done in regard to the visceral disabilities, as vomiting, constipation, incontinence of urine, although in regard

to the two first many indirect devices are familiar to physicians, and often employed.

The "auto-suggestion" to which authorities on hypnotism so often refer, and which is said to offer serious obstacles to hypnotic suggestion, can only be a certain "set" of the brain processes, or neurone combinations, effected by the record of previous experiences. It is difficult to see how any will process can have anything to do with the matter. Indeed, I fail to understand how the will intervenes either in the theory of hypnotism or in modifying the course of its phenomena. Perhaps one of the greatest indirect utilities which is to result from the study of hypnotism may be the abolition of the fantastic attempt to urge the control of nervous phenomena of central origin by an energetic effort of the will. To the extent to which such phenomena depend upon central impressions they may be combated by other impressions, spontaneously or artificially generated. But such impressions can not voluntarily or willfully be produced.

From the moment the organism has deviated from its normal state of forgetting the past in the incessant creation of the present which alone constitutes its sphere of consciousness, it has become dependent upon external aid for restoration. Happy he for whom such aid can be adequately secured!

The demonstration that the communication between the nerve elements or neurones of the nerve centres was effected by contact and not through continuity of structure, promptly and almost irresistibly suggested the hypothesis that this contact could be interrupted. Further, that through multiple rearrangements and combinations of contacts, and their interruptions, many physiological and pathological phenomena could be explained—thus sleep, hysterical restrictions of consciousness, etc. The doctrine has already been formulated by Lepine, who seems to have been the first to do so;[1] by Duval,[2] by Dercum,[3] who extends the theory to the explanation of hypnotic phenomena. I have not ventured to be so precise, but to use a more general formula which would remain applicable whether the new and fascinating theory of movable neurones be substantiated or not.

[1] Un cas d'hystérie. *Revue de méd.*, 1894, p. 727.

[2] Théorie histol. du sommeil. *Soc. biol.*, 1895, p. 74.

[3] *American Journal of the Medical Sciences*, 1896, cxii, 151. Ramón y Cajal admits passive movements by interposition of neuroglia cells.

ADDRESS BEFORE THE WOMENS MEDICAL ASSOCIATION ABOUT 1900.

[*First Page Lost.*]

THE disadvantage of individual feebleness is only to be palliated by means of the union of forces, and we must certainly know ourselves individually feeble enough to desire to unite. Unfortunately, it often happens with weaker parties that they intensify their own weakness by internal discords at the very moment that the closest union, the most frank and fraternal friendship, can alone save their cause and win the day. "Yes," said Benjamin Franklin, on a famous occasion, "we must all hang together or without doubt we shall all hang separately."

The imperfect cohesiveness, so apt to be observed among women, their imperfect sense of class interests and of the necessity of vigorous enthusiasm for their class, has often been explained by the traditional isolation of women from one another, each as the center of a family and called upon therefore to play her hand alone. Goethe commented on this many years ago, and considered it a reason that girls should never be dressed in uniform. I think, myself, that the lack of political rights and of the habitual exercise in masses of political duties, is largely responsible for the deficient ability for collective action which is so noticeable in women, a defect which is only slightly less marked among medical women than among others.

Yet we are doubly members of a class, and therefore set apart to support each other. We are first physicians, and then women physicians, a lesser circle within a greater, to be governed, however, by essentially the same principles. The great principle that in most modern times has revolutionized the position of the medical profession in society is that its members are expected

to be constantly engaged in the investigation of truth. The passive uncritical acceptance of old truths, is not sufficient. All over the civilized world thousands of eager and laborious minds are at work night and day to try to roll back the thick curtain of darkness which still covers so much: to try to wrench some new fragment of truth from the abysmal unknown. The medical profession has always existed, but it has not always been respected. I might say that it was not respected during the many ages when doctors did nothing but write prescriptions, when their business was supposed to be to treat disease, but not in the least to know anything about either disease or life. During the time, in a word, when the apothecaries who handled drugs and the barbers who handled knives, were supposed to have nothing in common with the physicists and philosophers who sought after truth. Now, do we not as women, somewhat tend to relapse into this status of antique medicine—to be very anxious to learn how to treat our cases, but rather indifferent to the vast mass of subtle and curious problems which modern medical science rolls in upon us? The practical evil of this indifference is that whatever class of persons is guilty of it, becomes in the long run incapable of practical success, and moreover, loses the reputation through which alone can come opportunities for practical effort. As I speak, however, I remember the work of real investigation that has been done by several of our colleagues: I recall Dr. Williams' researches on diphtheria; Dr. Wollstein's essay on the bacteriology of infantile diarrhœa; Dr. Baldwin's recent experiments on urea; the report on the extraordinary case of foetal icthyosis made at the American Medical Association by Drs. Cordes and Daniel; not to speak of that older case of Dr. McNutt on diplegia, which seems to have made the tour of the civilized world. Could we extend our membership to Baltimore, we could include Claribel Corre, whose case of brain sarcoma was a most brilliant contribution at the Philadelphia Alumnæ Association two or three years ago; and Florence Sabin, whose wonderful model of the microscopic structure of the brain, is a proof equally cogent and welcome of a solid intellectual capacity among women of a peculiarly rare order. I refer, without permission, to those among our colleagues to show, were it necessary, that we have with us the beginnings of everything which has made the medical profession at large useful, respected, rich and powerful. But the fact is not as yet

so well recognized by the world that we can afford to let it alone. The difficulty for women in this as in every other respect, lies, not in quality but in quantity, or in such defects in quality as result from defects in quantity. Our writings are apt to be thin, because they do not contain enough facts, do not go over enough ground, do not repose upon a rich enough background of literature. Now the richness of much contemporary medical writing is only obtained through the cooperative work of several people. Dr. Barker, of the Johns Hopkins, has just published a treatise on the nervous system, whose erudition, even in regard to the bibliography, is amazing when considered as the expression of the labors of a single man, much of whose time is employed otherwise than in the preparation of this book. But the professor was able to call upon colleagues, and pupils and students of the university to assist him. In Paris, when the candidates for hospital positions are writing the theses which constitute the most important competitive test, and which must be handed in, printed, a fortnight from the time the subject of them is given out, they summon all their friends to join in this preparation. Not one man, but a dozen are at work night and day reviewing the notes which have been previously collected during many years on all kinds of subjects, and so systematically arranged that they can be referred to at any moment. I should like to see this association organized, for part of its function, as a permanent committee of the whole for constant research. The first years after graduating from the medical school are very apt to be mentally wasted by young physicians who have failed to be caught up within the inner currents of an intellectual center. They want to do something but they do not know what to do. They forget that one employment is permanently open to every intelligent human being—the employment, namely, of learning something new, a definite series of new things. The field is so vast, it can never be exhausted, and the work is always exciting. To set out to learn something new, new for oneself, even if already known to the world, is quite a different enterprise from that of getting up an article for a medical journal, or even for the meetings of a medical society. The reason that there is apt to be so much difficulty in securing papers for meetings and that the efforts to sustain medical journals by women are apt to be such failures, is that the cart is put before the horse: the writing has not been preceded

by the steady, prolonged work which can alone collect such new facts or new views as should be worthy of publication. The article therefore if written, is liable to be merely a rehash of things which have been said a dozen times before.

I should propose that this association appoint a committee upon subjects for investigation covering many branches. We need to have a center for ourselves where some definite work is always going on, where a series of researches are constantly being made, and various classes of material being accumulated, into which any new comer may freely dip. Without pretending to be exhaustive I would suggest:

First: A series of embryological studies in the chick, indefinitely renewed, and resulting in numerous microscopic preparations to be accumulated in a museum for frequent reference. These series could be frequently interrupted by preparations from other animals, including the human, whenever a fortunate chance rendered these latter accessible.

Second: A series of studies in bacteriology.

Third: A similar series in hæmatology where the one item of the malarial parasite has just offered a multitude of new details for investigation.

Fourth: Continuous work in the preparation of a nervous tissue, both for normal and pathological anatomy. The great variety of new methods recently devised, and applicable to the tissues of freshly killed healthy animals afford endless food for thought and scope for practise, and the time required to prepare tissues for staining and section is so long, that the individual observer unless entirely devoted to this kind of work, is easily discouraged from undertaking it.

Fifth: I am not sure that it would be well at present to propose analogous physiological work involving vivisection. But there is a new and large field of clinical physiology, embracing experiments on human beings with various instruments of precision such as the plethysmograph, or instruments for graphic curves, and which require such a large number of observations as can only be fruitfully made by groups of observers working in concert. Experiments in psychophysics, for which Scripture's manual offers a reliable guide, would fall under this head.

Sixth: Records of groups of clinical facts in medicine and surgery, collected from the experience of the association, from the

books of hospitals and dispensaries, to which access had been obtained; reenforced by bibliographical research, the whole analyzed and summarized and kept, so to speak, on top, ready for use. I think this could be made one of the most popular of our departments. It seems to me that upon application, this association could secure the use of the laboratories in the old medical college. Upon what terms, I have not yet imagined. That would remain to be considered. But it was said in a general way, that these rooms should stand for the use of young physicians "wishing to do post-graduate work," and the plan I suggest is more voluminous and solid than that. It implies the creation of an intellectual atmosphere to be permanently breathed by all of us, for our constant and needed invigoration. It is only those who are constantly breathing such an atmosphere who can hope to progress in medicine, and to resist the tug of the mass of deadening influences which are constantly tending to drag them down into pettinesses and in the press of daily routine and anxieties, it is difficult for any isolated individual, especially if she be a woman, to break away and start afresh some new enterprise, which moreover she would be obliged to carry on alone by her unassisted strength. Much easier if the habit were once established as a matter of course to add daily to the other business of the day, some share in work already established and going on. To do this as the bees work, without any especial reference to individual advancement, but the intention of contributing to the upbuilding of the hive.

The evenings of our monthly meetings would upon this plan, be in part always devoted to some report from some stage of the continuous work. Room would be left, however, for any individual effort that should occur, but which would then have a permanent background of solid work with which to be contrasted and compared, and not as now is too frequently the case, have only a background of nothingness, and be obtained by means of laborous solicitation and conceded as a personal favor.

The meetings of the Hospital Section would on the other hand, be devoted entirely to personal experiences, as distinguished from systematic exposition of a given subject. These meetings should be like a day book as compared with a ledger.

There is a second great class of interests which our association is bound to consider, and which differs radically from the scien-

tific subjects we have been so far speaking about. In the pursuit of truth, and while engaging in work that may hope to demonstrate something new, we are able to think of ourselves simply as human beings. But when we consider our practical daily work in the treatment of the sick we very soon find that we are at a considerable disadvantage as compared with many other human beings. So long as the public hospitals are not open to women, women remain deprived of the fundamental indispensable basis of their entire work. I am often surprised that this unjust privation does not seem to excite among women in general the same vehement indignation which I feel about it myself. Here is where vigorous collective action would seem to be especially necessary. There are two kinds of hospitals to be considered, the large and the small. To the large hospitals, as notably the Woman's Hospital, admission must be sought by means of increasing pressure of public opinion. But to the small hospitals, which are poor and struggling and always in debt, there are frequently opportunities to obtain admission by the quid pro quo of purchase. It is indeed comical, in view of the lofty objections so commonly urged against the presence of women, to note how rapidly these melt down under the influence of a few thousand dollars. Two hundred thousand dollars purchased for women the permanent entrance to Cornell University; $100,000 threw open to them the inestimable advantages of the Johns Hopkins Medical School. I have a small hospital in view, where I believe that a donation of $10,000 would radically change the entire mental horizon, and permit what has hitherto been refused, the admission of women both to the staff of internes and also permanently to the visiting board. I do not want to speak in too much detail of this hospital on this occasion, for it would be deleterious to the success of the plan should it be known or talked about publicly in advance. But my plan is to try to persuade ten persons of whom I have several in mind, to contribute $1,000 a piece, or rather to make a promise of such contribution on condition that the hospital agree to always have one woman among its internes, admitted like the others after competitive examination, and also one woman on its board of visiting physicians. I think this association might pledge itself to obtain the promise of a thousand dollars of this donation, promise only to be redeemed in case the rest is raised. Should one hospital be secured for this

plan an immense step would have been gained, the beginning of an immense advance.

Many and contradictory are the reasons alleged to oppose the admission of women to the hospitals. But they all sift down to one; namely, the belief that it would be impossible to make a junior interne who was a young man, submit to the official superiority of a young woman who should have become senior. There is a dogged prejudice on this point, which must be overcome, and as there are no resources on hand to overcome it by force, it must. as on other analogous occasions, be bought off.

In disbanding the Association for the Advancement of the Medical Education of Women, I left it to be understood that the New York Medical Association of Women would undoubtedly appeal for assistance to the same friends who for so many years had been helping them in the old times. Our methods of appeal must now be different. Thirty years ago there were few graduated physicians and the undergraduates were on the curiously childish basis. The effect of the childish estimate in which they were generally held by their patrons was often perceptible in the managment of the infirmary and dispensary today, and since the organization of our present association, the women physicians of New York should claim to stand on their own feet, to work for their own interests, in the same resolute way in which, for instance, groups of foreign physicians have developed themselves into powerful organizations. The methods are always the same, definite aims, enthusiasm about these, willingness to make personal sacrifices to advance them, further willingness to advocate these among wide circles of friends, and endeavor to secure their support. And for the moment the two schemes I commend to your exertions are: first, the organization of a system for permanent research; and, second, the collection of $10,000 as a donation to a certain small hospital, to be given on the condition of admitting women physicians as internes and also permanently on the medical board.

DESCRIPTION OF THE EARLY SYMPTOMS OF THE MENINGEAL TUMOR COMPRESSING THE CEREBELLUM, FROM WHICH THE AUTHOR DIED. WRITTEN BY HERSELF.

. . . I DO not believe that any one ever had such good health, certainly not any better than I enjoyed until the age of 54 years. During this period, I sustained many trials, some of a nature peculiarly calculated to break down the nervous system of women, but I did not break down. Indeed I often reflected with pleasure that not only my muscles, digestive apparatus, etc., were always in perfect running order, but my brain was always singularly clear and buoyant. It seemed to me often as if I lived in a glass house on the summit of a lofty mountain where I could see in every direction an almost illimitable distance looking through an atmosphere of blue and gold. The delight I experienced in the clearness of this view was immense. On account of it I was never conscious of depression or of irritation for more than a few moments at a time. I lived in an equable golden calm as in a sunrise or sunset cloud. I emphasize this habitual condition because it was on account of it that the first symptoms of the present illness became so conspicuous from contrast and attracted my attention, as otherwise they might not have done.

In the winter of 1896, . . . on waking one morning I experienced a very sharp pain running transversely just below the occiput. It lasted between three and five minutes, then disappeared, and was heard no more of throughout the day. But the next morning at precisely the same time the suboccipital pain returned with precisely the same characters, and lasting precisely the same length of time. From this date, the same thing happened every

morning for four years, and the pain never occurred at any other time of the day. But in 1900 it did begin to come on occasionally at other times, always, however, lasting such a short time, three to five minutes, that it did not seem to me deserving of much attention, however severe it was while it lasted. Finally, in the year 1900, the morning pain instead of disappearing persisted and increased in severity, in extent, and territory, became complicated with nausea, then vomiting—assumed in fact all the characters of an ordinary sick headache.

I think I had never had a sick headache before. It lasted from early morning until early in the afternoon, then died away under the influence of phenalgine. This sick headache recurred every six months during the next year and a half. In the interval the head was as clear as usual, only the sharp attack of sub-occipital pain continued to occur for five minutes every morning.

In June of 1901, I joined a party in an expedition to the Yellowstone Park, where I spent a week. On the first day, and after the thirty-six hours' railroad ride, I had a sick headache with moderate pain, but much nausea. The next day, and for a week, I was perfectly well. But the day before leaving I indulged in a hot bath from the geyser water and was seized in the night with an extremely violent pain in the head, not limited as usual to its posterior third, but extending all over, and soon accompanied by retching and vomiting. These symptoms were so severe in the morning that I was quite unable to rise and accompany the party home. I remained in bed all day, took phenalgine, and gradually recovered. The following winter, however, I noted a gradual increase in the head symptoms occurring on waking in the morning, also a great difficulty in arising from a recumbent to a sitting position. Frequently there was an attack of nausea, and even vomiting, after getting out of bed. Great irritability of the bladder at this time, and that also frequently continued during the day, but altogether the first few hours in the morning were always pretty miserable. Nevertheless, I managed to do my work, usually after 10 A.M., and felt pretty well, but during the winter of 1901–1902, the attacks of sickness became more frequent, and towards spring about once every two or three weeks I was laid up in bed all day. At the end of May, I had an especially severe attack, which was arrested this time by nitroglycerine. I was then ordered to bed for a fortnight, the

first such experience in my life, during which time I felt quite ill. I continued to take a few tablets of nitroglycerine daily.

On the seventh of June, I went into the country with my family, and stayed there until the seventh of October. Three days before I went to bed I suddenly lost in great measure my power of walking. I was walking home, . . . when it suddenly seemed to have become almost impossible to drag one foot after another. It was with great difficuly that I climbed up the steps to the house. This was the first of June, and from that date to the present my walking power has been greatly reduced. During the summer I at first only attempted to walk a few steps off the piazza. After a month I could sometimes walk a quarter of a mile, and in three months I could occasionally walk half a mile. There was no pain or stiffness in the legs at the time, but since the last week I have noticed, with some concern, that a great feeling of heaviness and dragging is liable to come on in the end of the back, and especially after walking. I cannot now walk more than from three to six blocks. At the time of this limitation in extent of walking capacity, I began to totter somewhat in walking. On going downstairs there was a tendency to pitch forwards. This tottering has not increased, but it has persisted. Since the last two months, I cannot walk more than three or four blocks, and that with the aid of a cane and an assistant's arm. From time to time I have fallen suddenly—not when out of doors, most frequently upon arising after sitting for a long time, perhaps especially in the evening. I would fall to the floor, and experience considerable difficulty in getting upon a chair. The fall was unaccompanied by either vertigo, giddiness or pain. Indeed no different sensation in any part of the body: the legs simply gave way as if I had been on skates. After a moment or two, I could climb to my feet again and felt none the worse for the adventure. These attacks of falling have occurred about once in three or four weeks during the winter.

I do not find any symptoms of paralysis in any of the four limbs, either of motility or sensibility. Neither are there cramps, contracture of muscles, or stiffness in them. I can climb in and out of a bath tub usually with ease, though occasionally I need the maid to assist me, as also in arising from the bed in the morning. There is no steadiness or regular progression in this nor

in any of the other symptoms. There is a constant sense of general fatigue and inability for exertion.

A slight tremor has occurred in my right hand during the last winter. It comes during repose and is scarcely rhythmical; its excursion is very small; it is not developed during motion or exertion. During the last two or three weeks, I think this tremor has been much less, and often for many hours it would be entirely absent. There is no fibrillary tremor.

A change in mental condition began in a subtle manner about six years ago. I began to lose the initiative, which had formerly been so active with me. I was not at all depressed or melancholy, but became relatively indifferent. It seemed as if a fine gauze veil were thrown over all the objects in which I had formerly been so intensely interested. It was like the life after death as the Greeks understood it when they described Hades. My interest in contemplation persisted, and even increased, but I acquired an increasing reluctance to effort and voluntary mental exertion. I appreciated Schopenhauer's "Condemnation of the Will," and felt confirmed in my view that his entire theory sprang from a deep inner consciousness of personal weakness of volition. This impressed me the more from its sharp contrast with the vivacity and strength of volition which had been a leading characteristic with me all my life. There was a facility of fatigue after mental exertion, quite comparable to that after walking. This became marked at the same time with the latter, that is after June, 1903, although the sense of loss of initiative had begun, as I have said, six years before. In the last week I have had for the first time a dragging heaviness in my left arm, and some stiffness when I move it backward.

BIBLIOGRAPHY

Medical Matters in Paris. Letters signed P. C. M. *Med. Rec.*, 1867–68, ii, 212, 304, 351, 376, 476, 501, 547, 572; 1868–69. iii, 68, 114, 213, 306, 329, 428; 1869–70, iv, 44, 114, 280, 306, 548.

Some Details in the Pathogeny of Pyæmia and Septicæmia. *Med. Rec.*, 1872, vii, 73.

Anomalous Malformation of the Heart. *Med. Rec.*, 1872, vii, 111.

Intestinal Obstruction. *Med. Rec.*, 1872, vii, 208.

Address to the Graduating Class of the Woman's Medical College of the New York Infirmary. *Med. Rec.*, 1872, vii, 215.

Thrombosis of Ovarian Veins. *Med. Rec.*, 1872, vii, 305.

Scarlatinous Nephritis. *Med. Rec.*, 1872, vii, 354.

Notes on a Case of Human Nosencephalion Monster which lived Twenty-nine Hours. *Arch. Sc. and Pract. Med.*, 1873, i, 342, 446.

Phenomena Attending Section of the Right Restiform Body, *Med. Rec.*, 1873, viii, 17.

A Case of Malignant Icterus. *Med. Rec.*, 1873, viii, 65.

Enormous Fatty Degeneration of the Liver, *Med. Rec.*, 1873, viii, 94.

Lecture on Atropin. *Med. Rec.*, 1873, viii, 249, 273.

Ovarian Tumor. *Med. Rec.*, 1873, viii, 342.

Pericarditis in a Child Eight Years Old. *Med. Rec.*, 1873, viii, 624.

Note on the Significance of the Pulse in the Forming Stage of Generalized Puerperal Peritonitis. *Arch. Sc. and Pract. Med.*, 1873, i, 51.

Hemorrhage into the Ovisacs During Pneumonia. An Interesting Question in Pathology. *Med. Rec.*, 1874, ix, 410.

Pathogeny of Infantile Paralysis. *Am. Jour. Obst.*, 1874, vii, 1.

Nitrite of Amyl and Belladonna in Dysmenorrhœa. *Med. Rec.*, 1875, x, 11.

General Miliary Tuberculosis and Endometritis. *Med Rec.*, 1875, x, 27.

The Death of Doctor Dimock. *Med. Rec.*, 1875, x, 357, 358.

Remarks upon the Action of Nitrate of Silver on Epithelial and Gland Cells. *Trans. New York State Med., Soc.*, 1875, 251.

Puerperal Fever, Infection from Ovary through Retroperitoneal Glands. *Med. Rec.*, 1876, xi, 397.

Monthly Abortions. *Med. Rec.*, 1876, xi, 481.

The Treatment of Hydrophobia by Woorara. *Trans. New York State Med. Soc.*, 1877, 23.

Croup and Diphtheria. *Med. Rec.*, 1877 xii, 125.

Cerebrospinal Meningitis—Pseudomembranous Croup. *Med. Rec.*, 1877, xii, 237.

Sudden Death without Apparent Cause. *Med. Rec.*, 1877, xii, 476.

Two Cases of Convulsive Disease without Convulsions. *Trans. New York State Med. Soc.*, 1877, 147.

Sphygmographic Experiments upon a Human Brain Exposed by an Opening in the Cranium. *Am. Jour. Med. Sc.*, 1878, lxxvi, 10.

Curious Congenital Deformities of Upper and Lower Extremities. *Med. Rec.*, 1878, xiii, 115.

Acute Fatty Degeneration of the New-born. *Am. Jour. Obst.*, 1878, ii, 499.

Aphasia Due to Thrombosis of Small Twigs Supplying the Speech Region. *Med. Rec.*, 1879, xv, 425.

Note on the Cause of Sudden Death during the Operation of Thoracentesis. *Med. Rec.*, 1879, xvi, 139, 331.

Fatty Degeneration of the Placenta. *Med. Rec.*, 1879, xvi, 162.

Generalized Tuberculosis. *Med. Rec.*, 1879, xvi, 570.

Malignant Icterus with Great Enlargement of the Liver. *Trans. New York Path. Soc.*, 1879, iii, 50.

Scarlatinous Nephritis. *Trans. New York Path. Soc.*, 1879, iii, 180.

Provisional Report on the Effect of Quinine upon the Cerebral Circulation. *Arch. Med.*, 1879, i, 33.

Contributions to Sphygmography. The Influence of Pain on the Pulse Trace. *Arch. Med.*, 1879, ii, 51.

Cystic Calculi. Spasmodic Action of Gall Bladder. *Med. Rec.*, 1880, xvii, 238.

Suppurative Fibrinous Pleurisy Supervening upon Broncho-pneumonia in a Child. *Med. Rec.*, 1880, xvii, 271.

Case of Uterine Fibroid Treated by Ergotine Injections and Finally Removed by means of Thomas's Scoop. *Am. Jour. Med. Sc.*, 1880, *n.s.* lxxix, 422.

Case of Tubercular Meningitis with Measurements of Cranial Temperatures. *Jour. Nervous and Mental Dis.*, 1880, vii, 51.

Scarlatinous Nephritis. *Med. Rec.*, 1881, xix, 353.

Case of Microcephalus. *Med. Rec.*, 1881, xix, 645.

Pericarditis in a Child Eight Years. *Trans. New York Path. Soc.*, 1881, iv, 9.

Review of Fever: a Study of Morbid and Normal Physiology, *Arch. Med.*, 1881, v, 302.

A Case of Trephining of Sternum for Osteomyelitis *Am. Jour. Obst.*, 1881, xiv, 981.

Case of Facial and Palatine Paralysis and Loss of Equilibrium Produced by a Fall on the Head. *Independent Pract.*, 1881, ii, 69.

Inaugural Address at the Opening of the Woman's Medical College of the New York Infirmary, October 1, 1880, *Chicago Med., Jour. and Exam.*, 1881, xlii, 561.

Desquamative Pneumonia, Tubercular Enteritis. *Med. Rec.*, 1882, xx., 410.

Specialism in Medicine. Editorial Department, *Arch. Med.*, 1882, vii, 87.

Shall Women Practice Medicine? *North American Rev.*, 1882, cxxxiv, 52.

Endocarditis—Embolic Pneumonia. *Med. Rec.*, 1883, xxiii, 301.

Spina Bifida Complicated with Hydrocephalus. *Med. Rec.*, 1883, xxiii, 357.

An Address Delivered at the Commencement Exercises of the Woman's Medical College of the New York Infirmary, May 30, 1883. *Arch. Med.*, 1883, x, 59.

Opening Lecture on Diseases of Children, at the Post-Graduate Medical School. *Boston Med. and Surg. Jour.*, 1883, cviii, 121, 145.

Aspiration of Dermoid Cysts Followed by Inflammation. *Am. Jour. Obst.*, 1883, xvi, 1160.

Cystic Ovaries; Battey's Operation. *Med. Rec.*, 1884, xxv, 705.

Salpingo-oophorectomy. *New York Med. Jour.*, 1884, xxxix, 673.

Some Considerations on Endometritis. *Boston Med. and Surg. Jour.*, 1884, cx, 468.

Two Peculiar Cases of Typhoid Fever; One at the Age of Six Months, One beginning with Pneumonia and Heart Failure Conspicuous. Effect of Digitalis. *Arch. Med.*, 1884, xi, 30–72.

Case of Aortic Stenosis Followed by Left Hemiplegia in a Child of Six. *Arch. Pediat.*, 1885, ii, 710.

Studies in Endometritis. *Am. Jour. Obst.*, 1885, xviii, 36, 113, 262, 519, 596

Progress in Medical Education. The Women Taking the Lead. *Med. Rec.*, 1885, xxviii, 584.

Theories of Menstruation. New Theory. (Studies in Endometritis.) *Am. Jour. Obst.*, 1885, xviii, 376.

Morbid Variations in the Greater or Parturient Cycle, Subinvolution and Chronic Metritis. (Studies in Endometritis.) *Am. Jour. Obst.*, 1885, xviii, 802.

Menstrual Subinvolution or Metritis of the Non-parturient Uterus. (Studies in Endometritis.) *Am. Jour. Obst.*, 1885, xviii, 915.

Pulmonary Consolidation; Enlarged Bronchial Glands; Pressure upon one of the Recurrent Laryngeal Nerves; Heart Clot. *Med. Rec.*, 1886, xxix, 401.

Dilatation and Fatty Degeneration of the Heart; Blood Changes of Pernicious Malarial Fever. *Med. Rec.*, 1886, xxx, 106.

Faradic Electricity in Rigidity of Os Uteri. *Am. Jour. Obst.*, 1886, xix, 36.

The Ovarian Complication of Endometritis. (Studies in Endometritis.) *Am. Jour. Obst.*, 1886, xix, 352.

Infantile Spinal Paralysis. In a System of Practical Medicine by American Authors, edited by William Pepper, M.D., LL.D., 1886, v, 1113.

Profuse Epistaxis: Dilatation and Fatty Degeneration of the Heart. *New York Med. Jour.*, 1886, xliii, 669.

Pseudo-hypertrophic Paralysis. In a System of Practical Medi-

cine by American Authors, edited by William Pepper, M.D., LL.D., 1886, iv, 557.
Note on Empyema. *Med. Rec.*, 1887, xxxi, 634.
The Indication for Quinine in Pneumonia. *New York Med. Jour.*, 1887, xlv, 589, 620.
Dermoid Cyst by Inclusion. *Proc. New York Path Soc.*, 1887–88, 133.
Case of Post-epileptic Hysteria; Effect of Inhalations of Compressed Air; Phenomena of Transfer. *Jour. Nervous and Mental Dis.*, 1888, xv, 442.
Remarks on Cocculus Indicus. *New York Med. Jour.*, 1888, xlviii, 29.
The Nature and Dangers of Intra-uterine Medication. *Med. Rec.*, 1888, xxxiii, 23.
Notes on Uterine Versions and Flexions. *Am. Jour. Obst.*, 1888, xxi, 225.
Case of Uterine Fibroid Treated by Apostoli's Method; Enucleation of the Tumor. *Am. Jour. Obst.*, 1888, xxi, 806.
Case of Probable Tumor of the Pons. *Jour. Nervous and Mental Dis.*, 1889, xvi, 115.
Intra-uterine Therapeutics. *Am. Jour. Obst.*, 1889, xxii, 449.
Limitations and Dangers of Intra-uterine Medication. *Am. Jour. Obst.*, 1889, xxii, 598, 697.
Case of Cirrhosis of the Liver with Splenic Tumor. *Arch. Pediat.*, 1889, vi, 273.
Stomach Washing. *Proc. Alumnæ Assoc. Woman's Med. College of Pa.*, 1889, 50.
The Use of Electricity in Gynæcology. *Proc. Alumnæ Assoc. Women's Med. College of Pa.*, 1889, 60.
Practical Study of Biology. *Boston Med. and Surg. Jour.*, 1889, cxx, 631.
Acute Mania after Operations. *Med. Rec.*, 1889, xxxv, 446.
Case of Typhoid Fever Treated by Cold Baths. *Times and Register*, 1890, vii, 34.
A Plea for Medical Women. *Med. Rec.*, 1890, xxxvii, 107.
Female Physicians for Insane Women. *Med. Rec.*, 1890, xxxvii, 543.
The Temptation of Johns Hopkins. *Med. Rec.*, 1890, xxxviii, 588.
Myelitis, Spinal Meningitis and Hemorrhage into the Spinal

Cord or Membranes. In: *Cyclopædia of the Diseases of Children*, edited by John M. Keating, M.D., 1890, iv, 634.

Supernumerary Nipple. *Proc. New York Path. Soc.*, 1890, 41.

Hysterical Fever. *Jour. Nervous and Mental Dis.*, 1890, xvii, 373.

The Higher Education of Women. *Med. News*, 1890, lvi, 75.

Remarks on Empyema. *Med. News*, 1890, lvi, 117, 136, 170.

A Case of Infantile Pneumonia Traversing Three Dangerous Crises. *Arch. Pediat.*, 1892, ix, 118.

Urethral Irritation. *Proc. Philadelphia County Med. Soc.*, 1892, xiii, 450.

New Method of Recording Cases of Lateral Curvature. *Internat. Med. Mag.*, 1892, i, 307.

A Case of Myelitis with Vertebral Tumor. *Internat. Med. Mag.* 1892, i, 449.

Permanent Drainage in the Treatment of Endometritis. *Woman's Med. Jour.*, 1893, i, 1.

Modern Female Invalidism. *Boston Med. and Surg. Jour.*, 1895, cxxxiii, 174.

Case of Unilateral Congenital Ptosis. *Med. Rec.*, 1895, xlvii, 227.

Diseased Placenta. *Proc. New York Path. Soc.*, 1895, 45.

The Stephenson Wave. *Am. Jour. Obst.*, 1895, xxxii, 90.

Case of Absent Uterus with Considerations on the Significance of Hermaphroditism. *Am. Jour. Obst.*, 1895, xxxii, 510.

The Needs and Shortcomings of Women Physicians. *Boston Med. and Surg. Jour.*, 1895, cxxxiii, 146.

Ataxia in a Child. *Med. Rec.*, 1897, li, 761.

Experiments on Urinary Toxicity. *Med. Rec.*, 1897, lii, 653.

Considerations on Flechsig's "Gehirn und Seele." *Jour. Nervous and Mental Dis.*, 1897, xxiv, 747.

The Blood Count in Anemia and Certain Nervous Affections. *Med. Rec.*, 1898, liii, 933.

A Suggestion in Regard to Suggestive Therapeutics. *New York Med. Jour.*, 1898, lxvii, 485.

Varieties of Nephritis. *Woman's Med. Jour.*, 1898, vii, 191.

Myelitis, Meningitis and Spinal Hemorrhage. In: *Cyclopædia of the Diseases of Children*, edited by William A. Edwards, M.D., 1899, v, Suppl., 1206.

Women in Medicine. In: *Woman's Work in America*, edited by Annie Nathan Meyer, 1891.

A Case of Myosarcoma of the Uterus. *Am. Jour. Obst.*, 1902, xlv, 218. With Martha Wollstein, M.D.

BOOKS

Thesis, *De la Graisse Neutre et de les Acides Gras*, Paris, 1871.

The Question of Rest for Women during Menstruation. Boylston Prize Essay for 1876.

The Value of Life, 1879.

On the Use of the Cold Pack Followed by Massage in the Treatment of Anæmia, 1880. With Victoria A. White, M.D.

Essays on Hysteria, Brain Tumor and Some Other Cases of Nervous Disease, 1888. (Containing: Some considerations on hysteria; Tumors of the brain; Note on the special liability to loss of nouns in aphasia; Case of nocturnal rotary spasm; The prophylaxis of insanity; Antagonism between medicines and between remedies and diseases; Hysterical locomotor ataxia.)

Physiological Notes on Primary Education and the Study of Language, 1889.

Common Sense Applied to Woman Suffrage, 1894.

Stories and Sketches.

Edited by Dr. Mary Putnam Jacobi:

Infant Diet, by Abraham Jacobi, M.D., 1874.

Manual of the Domestic Hygiene of the Child, by Julius Uffelman, M.D., 1891.

INDEX

L

M

N

Q

R

S

Z

Jacobi, Mary P., *Mary Putnam Jacobi, M.D., a pathfinder in medicine...*, W7 J166m 1925

Condition when received: The book has undergone extensive conservation in the past. It has been entirely resewn and placed in a new buckram casing. A number of pages bear complex tears and small losses along the foredge and the bottom edge--to include the title page, table of contents and eight pages in the prologue.

Conservation treatment: In preparation for exhibition, the tears and losses were mended using acrylic-toned kizukishi and tengujo papers (Japanese Paper Place) and secured with a mixture of wheat starch paste (Talus) and methyl cellulose (A4M, University Products).

Treatment carried out by Rachel-Ray Cleveland
HMD Paper Conservator, 05/2003.